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ABSTRACT

The New Jersey Unemployment Insurance Reemployment Demonstration Project was undertaken to examine the feasibility of using the unemployment insurance (UI) system to identify displaced workers early in their unemployment spells and then accelerating their return to work by providing them with alternative, early intervention services. Three packages of services were tested: job search assistance (JSA), JSA combined with training or relocation assistance, and JSA combined with a cash bonus for early reemployment. Each treatment's cost-effectiveness was examined through a follow-up study examining long-run treatment impacts approximately 6 years after program participants had submitted their initial UI claims. The UI system was confirmed to be an effective means of early identification of displaced workers likely to experience long-run employment difficulties. All three treatments were found to result in jobs that were more stable than the jobs obtained by control group members. All three treatments afforded net benefits to claimants and society as a whole. (Twenty-eight tables/figures are included. Appended are a description of the samples used to estimate the profiling models and treatment/control group means. Also included is a brief report summarizing the demonstration findings and their policy implications. Contains 12 references.) (MN)

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The New Jersey Unemployment Insurance Reemployment Demonstration Project: Six-Year Follow-Up and Summary Report

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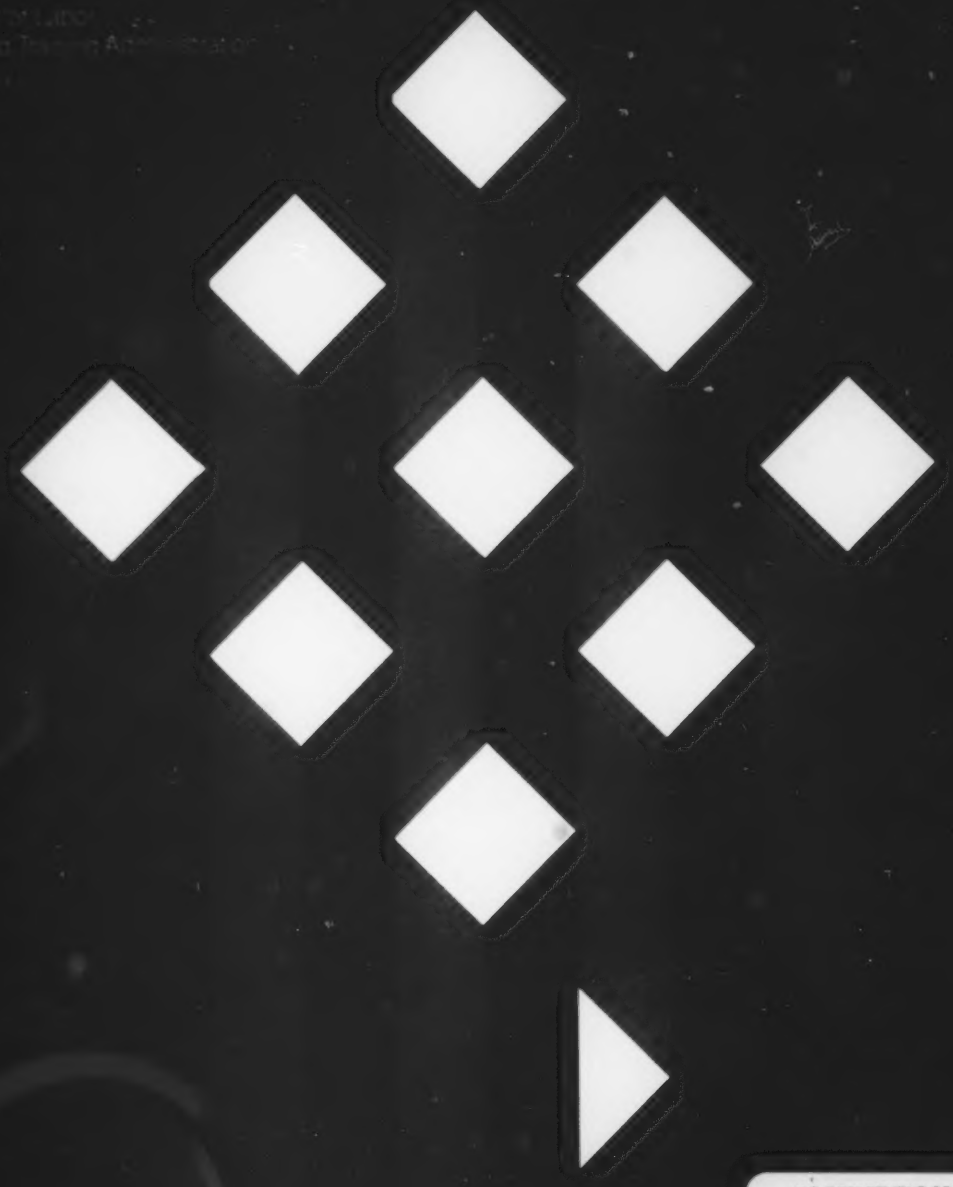

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The New Jersey Unemployment Insurance Reemployment Demonstration Project: Six-Year Follow-Up and Summary Report Revised Edition

Unemployment Insurance
Occasional Paper 96-2

U.S. Department of Labor
Robert B. Reich, Secretary
Employment and Training Administration
Doug Ross, Assistant Secretary

Unemployment Insurance Service
Mary Ann Wyrsch, Director

1996

This report was prepared for the Unemployment Insurance Service, U.S. Department of Labor under contract number P39803 with Mathematica Policy Research Inc. The authors of this report are Walter Corson and Joshua Haimson.

This report is a six-year follow-up to the report *The New Jersey Unemployment Insurance Reemployment Demonstration Project (UI Occasional Paper 89-3).* This second follow-up study extended the analysis further, thus allowing more definitive measures of demonstration impacts. THIS REPORT IS A REVISED VERSION OF UI OCCASIONAL PAPER 95-2 AND WILL REPLACE IT.

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THE NEW JERSEY UNEMPLOYMENT INSURANCE REEMPLOYMENT DEMONSTRATION PROJECT

SIX-YEAR FOLLOWUP AND SUMMARY REPORT

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Authors:

Walter Corson
Joshua Haimson

Submitted to:

N.J. Department of Labor
Labor Research and Analysis
Division of Program Planning, Analysis and Evaluation
Trenton, NJ 08625

U.S. Department of Labor
Employment and Training Administration
Unemployment Insurance Service
200 Constitution Avenue, N.W.
Washington, DC 20210

Submitted by:

Mathematica Policy Research, Inc.
P.O. Box 2393
Princeton, N.J. 08543-2393
(609) 799-3535

Principal Investigator:
Walter Corson

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At Mathematica Policy Research, Joshua Haimson was responsible for the analysis and for writing Chapters III and IV. Dexter Chu and Douglas Almond were responsible for the programming and helped develop the profiling models; their excellent work is reflected in all of the reports' tables. Paul Decker provided helpful comments on the report. Cindy Castro, who oversaw production of the final report, generated a very attractive product under a tight timetable, and Joanne Pfleiderer edited the report, turning awkward drafts into user friendly text.

Walter Corson
Project Director

PREFACE

This report on the New Jersey Unemployment Insurance Demonstration Project contains two sections: (1) the six-year follow-up report which focuses on demonstration impacts on UI receipt and employment and earnings over six years and (2) a short report which summarizes the demonstration findings and discusses their policy implications. While these reports are published together here, they were prepared as stand-alone documents intended for different audiences.

The initial version of this report, which was published as Unemployment Insurance Occasional Paper 95-2, contained a specification error in the analysis of impacts for profiled and nonprofiled workers, which was presented in Chapter IV.C. This version of the report corrects this error and presents new estimates of impacts for profiled and nonprofiled workers. The impact estimates for profiled workers are larger than those reported previously.

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PART I

THE NEW JERSEY UNEMPLOYMENT INSURANCE REEMPLOYMENT DEMONSTRATION PROJECT

SIX-YEAR FOLLOWUP

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EXECUTIVE SUMMARY

The purpose of the New Jersey Unemployment Insurance Reemployment Demonstration Project (NJUIRDP) was to examine whether the Unemployment Insurance (UI) system could be used to identify displaced workers early in their unemployment spells and to provide them with alternative, early intervention services to accelerate their return to work. Three packages of services, or treatments, were tested in the demonstration: (1) job-search assistance (JSA) only; (2) JSA combined with training or relocation assistance; and (3) JSA combined with a cash bonus for early reemployment. A key component of the demonstration was that eligible claimants were identified and services were provided through the coordinated efforts of the UI, Employment Service (ES), and Job Training and Partnership Act (JTPA) systems. Another key element was that claimants were required by UI to report for services; failure to report could lead to the denial of benefits.

The demonstration was initiated by the U.S. Department of Labor (USDOL) through a cooperative agreement with the New Jersey Department of Labor. It began operations in July 1986, and, by the end of sample selection in June 1987, 8,675 UI claimants were offered one of the three service packages. Services to eligible claimants continued into fall 1987 to ensure that all eligibles who wanted the full set of demonstration services were able to receive them. Another 2,385 claimants receiving existing services provided a control group for comparative purposes. Claimants were assigned randomly to this control group or to one of the three treatments.

The initial evaluation of the demonstration (Corson et al. 1989), combined with a follow-up study that extended the analysis for approximately three years after the initial UI claim (Anderson et al. 1991), found that each of the treatments reduced UI collections and increased employment and earnings during this period. Although the initial evaluation found no evidence that the training component of the second treatment increased earnings in the year after the initial UI claim, the follow-up study suggested that training did increase earnings in the longer run. Finally, the evaluation found that all three treatments offered net benefits to society, when compared with existing services. The JSA-only and JSA plus reemployment bonus treatments also led to net gains for the government.

This second follow-up study extended the analysis for approximately six years after the initial UI claim to identify any long-run treatment impacts, particularly for trainees. This second follow-up also provided an opportunity to examine displaced workers' long-run earnings patterns, to determine if the method used in the demonstration was successful in identifying displaced workers who experienced long-run employment difficulties. Alternative methods of identifying displaced workers were also investigated.

This follow-up evaluation found additional long-run UI impacts suggesting that each component of the treatments--JSA, training, and the reemployment bonus--probably contributed to the longer-term impacts and that the treatments, in general, generated jobs that were more stable than those found by control group members. It also suggests that the NJUIRDP succeeded in targeting claimants who in the absence of the demonstration, would have experienced more severe long-run reemployment difficulties.

UI RECEIPT AND EMPLOYMENT AND EARNINGS

Overall, each NJUIRDP treatment reduced the amount of UI benefits received, both in the initial benefit year and in subsequent years. Statistically significant reductions in UI benefits occurred in the year after the initial benefit year for the JSA-only and JSA plus reemployment bonus treatments, in the next year for the JSA plus reemployment bonus treatment, and in the recent Emergency Unemployment Compensation (EUC) program for the JSA plus training or relocation assistance treatment. Over all UI programs, the NJUIRDP treatments reduced UI benefit receipt by about three-quarters of a week for the JSA-only treatment, by one and a half weeks for the JSA plus training or relocation assistance treatment, and by nearly two weeks for the JSA plus reemployment bonus treatment. These findings suggest that each of the treatment components--JSA, training, and the reemployment bonus--probably contributed to the longer-term impacts and that the treatments, in general, generated jobs that were more stable than those found by control group members. This finding differs from the first follow-up finding, which attributed longer-run impacts solely to the JSA component of the treatments.

Analysis of employment and earnings following the initial UI claim suggests that at least one treatment, the JSA plus reemployment bonus, increased earnings initially. None of the treatments had statistically significant longer-run impacts on the probability of working, the amount of earnings, or weeks worked. However, since the variation in earnings among claimants is quite large, modest earnings impacts consistent with the UI impact estimates could still have occurred.

An examination of earnings for employed control group members showed that nominal annual earnings remained below base-period levels until the fourth year after the initial UI claim. Even by the sixth year, earnings for employed individuals had not kept pace with inflation. These findings suggest that, on average, claimants were unable to obtain reemployment in jobs with the same earnings potential as that of their pre-UI jobs.

IMPACTS OF TRAINING

Participation in training was expected to increase the long-run earnings of trainees, yet comparisons of the earnings impacts of the JSA plus training or relocation treatment with those of the JSA-only treatment suggest that the training component had no additional impact. However, only a relatively small number of claimants participated in training, so the impacts of training would need to be quite large to be detected. Thus, we examined the earnings experiences of trainees directly to determine whether their pattern of earnings suggested that training may have had an impact not detected in the treatment group comparison. This analysis suggested that both classroom (occupational skills) and on-the-job training did enhance trainees' earnings.

TARGETING OF SERVICES

The demonstration's eligibility screens succeeded in identifying a group of UI claimants (the control group is used for this analysis) that experienced relatively greater reemployment problems in the short term--as reflected by the number of weeks of employment and UI receipt in the first year of followup. During the full six years of followup, the group targeted by NJUIRDP continued to experience large reductions in earnings relative to their base-year earnings. These earnings reductions were considerably larger than those realized by noneligibles. However, the long-term UI

receipt of NJUIRDP eligibles was significantly smaller than that of noneligibles, a finding that can be attributed to the fact that workers in seasonal industries were among the noneligible population.

Based in part on the design and the initial findings from the NJUIRDP, the Unemployment Compensation Amendments of 1993 mandated that states identify workers likely to exhaust UI and refer them to reemployment services. USDOL has suggested that this targeting process, known as "worker profiling," can occur in a number of ways. One of the principal options involves screening out workers who are not permanently separated, estimating each individual's probability of exhausting UI, and serving those with the largest predicted probabilities of exhaustion.

In simulations of this targeting process that are representative of current funding levels, we found that the group targeted by profiling experienced somewhat greater reemployment problems than the NJUIRDP eligibles, as reflected in both groups' employment and UI receipt. These differences were apparent not only in the year following their initial claims but also during the full six years of followup. On the other hand, the group that would be served under profiling experienced smaller earnings reductions relative to pre-UI earnings than did the NJUIRDP eligibles, a finding that is probably due to the fact that the targeted group had fewer years of job tenure than the NJUIRDP eligibles. When we examined differences in impacts of the New Jersey treatments among workers targeted or not targeted by profiling, we found some evidence that treatment impacts were higher for the targeted group, particularly UI impacts. This finding suggest that using a profiling model to target reemployment services on workers with high probabilities of UI benefit exhaustion directs reemployment services to a group of workers who are likely to benefit from the services. These estimates also imply that this approach to targeting services is a relatively efficient way to provide services. Services are directed to a specific group of displaced workers who can benefit more from the services than a broader group of displaced workers, thereby generating relatively large savings in UI receipt for the given level of expenditures on services.

BENEFIT-COST ANALYSIS

The results of the benefit-cost analysis suggest that all three treatments offered net benefits to claimants and to society as a whole, relative to existing services. The JSA-only treatment and the JSA plus reemployment bonus treatment also led to net gains for the government sector as a whole and for the Labor Department agencies. The JSA plus training or relocation treatment was expensive for the government sector.

These findings suggest that it may be possible to fund the JSA-only and the JSA plus reemployment bonus treatments from the savings in UI benefits and increased UI tax collections. Our estimates indicate that the JSA-only treatment would pay for itself from the perspective of the Labor Department, while the JSA plus reemployment bonus treatment would lead to modest net benefits for the Labor Department. On the other hand, the JSA plus training or relocation treatment could not be funded solely from the savings in UI benefits and increased UI tax collections. It would require either a reduction in funding for other programs or an increase in taxes, because it appears to create net costs to the government as a whole.

I. INTRODUCTION

The New Jersey Unemployment Insurance Reemployment Demonstration Project (NJUIRDP) examined whether the Unemployment Insurance (UI) system could be used to identify displaced workers early in their unemployment spells in order to provide them with alternative, early intervention services to accelerate their return to work. Three packages of services, or treatments, were tested in the demonstration: (1) job-search assistance (JSA) only, (2) job-search assistance combined with training or relocation assistance; and (3) job-search assistance combined with a cash bonus for early reemployment. A key component of the demonstration was that eligible claimants were identified and services were provided through the coordinated efforts of the UI, Employment Service (ES), and Job Training Partnership Act (JTPA) systems. Another key element was that UI required claimants to report for services; failure to report could lead to the denial of benefits.

The demonstration was initiated by the U.S. Department of Labor (DOL) through a cooperative agreement with the New Jersey Department of Labor. It began operations in July 1986, and, by the end of sample selection in June 1987, 8,675 UI claimants were offered one of the three service packages. Services to eligible claimants continued into fall 1987 to ensure that all eligibles could receive the full set of demonstration services. Another 2,385 claimants who were receiving existing services served as a control group for comparison purposes. All eligible claimants were assigned randomly to one of the three treatments or the control group.

The initial evaluation of the demonstration (Corson et al. 1989), combined with a follow-up study that extended the analysis for approximately three years after the initial UI claim (Anderson et al. 1991), found that each of the treatments reduced UI collections and increased employment and earnings during this period. Although the initial evaluation found no evidence that the training component of the second treatment increased earnings in the year after the initial UI claim, the follow-up study suggested that training did increase earnings in the longer run. Finally, the evaluation

found that all three treatments offered net benefits to claimants and to society, when compared with existing services. The JSA-only and JSA plus reemployment bonus treatments also led to net gains for the government.

This second follow-up study extends the analysis for approximately six years after the initial UI claim to identify any long-run treatment impacts, particularly for trainees. This second follow-up also provides an opportunity to examine displaced workers' long-run earnings patterns to determine if the method used in the demonstration was successful in identifying displaced workers who experienced long-run employment difficulties. Alternative methods of identifying displaced workers are also investigated.

This follow-up evaluation found additional long-run impacts suggesting that each component of the treatments--JSA, training, and the reemployment bonus--probably contributed to the longer-term impacts. The followup also suggests that the treatments, in general, generated jobs that were more stable than those found by control group members. It also suggests that the NJUIRDP succeeded in targeting claimants who, in the absence of the demonstration, would have experienced more severe long-run reemployment difficulties.

The remainder of this chapter provides a brief synopsis of the NJUIRDP design, a summary of the findings from the initial and first follow-up evaluations, and a discussion of the purpose and design of the second follow-up study. A final section provides an outline for the remainder of the report.

A. SUMMARY OF THE NJUIRDP DESIGN

The NJUIRDP addressed three objectives: (1) to examine the extent to which UI claimants who might benefit from the provision of employment services could be identified early in their unemployment spells; (2) to assess the policies and adjustment strategies that were effective at helping such workers become reemployed; and (3) to examine how such a UI reemployment program should be implemented. To achieve these objectives, the design called for identifying demonstration-

eligible individuals in the week after their first UI payment and assigning them randomly to one of three treatment groups offering alternative packages of reemployment services or to a control group receiving existing services. The demonstration was implemented in 10 sites corresponding to state UI offices. The sites were chosen randomly, with the probability of their selection proportional to the size of the UI population in each office, yielding a sample representative of UI recipients in New Jersey.

1. Definition of Eligibility

The purpose of the demonstration was to provide reemployment services to experienced workers who, having become unemployed through no fault of their own, were likely to face prolonged spells of unemployment. Their job-finding difficulties might be due to unavailability of jobs, a mismatch between their skills and job requirements, or lack of job-finding skills. However, because previous research efforts have not established good predictors of prolonged unemployment spells, complex eligibility requirements could not be used to direct demonstration services. Because of this situation, the demonstration plan incorporated a small number of sample screens to identify experienced workers who were likely to be displaced permanently from their jobs.

The following eligibility screens were chosen for the demonstration:

- ***First Payment***. The demonstration excluded claimants who did not receive a first UI payment. To promote early intervention, the demonstration also excluded claimants who did not receive a first payment within five weeks after the initial claim. Individuals who were working and, consequently, who received a partial first payment were also excluded, because their job attachment meant that they had not necessarily been displaced. Finally, special claims (for example, unemployment compensation for ex-servicemembers or federal civilian employees, interstate claims, and combined wage claims) were also excluded.

- ***Age***. An age screen was applied to eliminate the broad category of young workers who have traditionally shown limited attachment to the labor market and whose employment problems may be quite different from those of older, experienced workers. This screen excluded workers under age 25 from the demonstration.

- ***Tenure***. Demonstration-eligible claimants had to exhibit a substantial attachment to a job, so that the job loss was likely to be associated with one or more of the reemployment difficulties described earlier. Each claimant was required to have worked for his or her last employer for three years prior to applying for UI benefits and could not have worked *full-time* for any other employer during the three-year period. The three-year requirement is used by DOL's Bureau of Labor Statistics to define displaced workers (Flaim and Sehgal 1985).

- ***Temporary Layoffs***. The demonstration treatments were not intended for workers who were temporarily laid off. Thus, it was desirable to exclude claimants on temporary layoff. However, previous research and experience show that some claimants say that they expect to be recalled, even when their chances of actual recall are slim. To ensure that these individuals were not excluded from the demonstration, only individuals who both expected to be recalled and had a *specific* recall date were excluded.

- ***Union Hiring-Hall Arrangements***. Individuals who are typically hired through union hiring halls exhibit a unique attachment to a specific labor market and were thus excluded from the demonstration.

2. The Treatments

As stated earlier, the demonstration tested three treatment packages for enhancing reemployment. Eligible claimants were assigned randomly to a control group that received existing services or to one of the three treatment groups: (1) JSA only; (2) JSA plus training or relocation; and (3) JSA plus a reemployment bonus.

The initial components of all three treatments were the same: notification, orientation, testing, a job-search workshop, and an assessment/counseling interview. These services were delivered sequentially, early in claimants' unemployment spells. First, a notification letter was sent to claimants in about the fourth week after they filed initial claims. Claimants usually began to receive services during their fifth week of unemployment. Services began when they reported to a demonstration office (usually an ES office) and received orientation and testing during a one-week period. In the following week, they attended a job-search workshop, consisting of five half-day sessions, and a follow-up, one-on-one counseling/assessment session scheduled for the subsequent week. These initial treatment components were mandatory; failure to report could lead to the denial of UI benefits.

Beginning with the assessment/counseling interview, the nature of the three treatments differed. In the JSA only group, claimants were told that, as long as they continued to collect UI, they were expected to maintain periodic contact with the demonstration office, either directly with staff to discuss their job-search activities or by engaging in search-related activities at a resource center in the office. The resource center offered job-search materials and equipment, such as job listings, telephones, and occupational and training literature. Claimants were encouraged to use the center actively and were told that, if they did not come to the office periodically, ES staff would contact them and ask them to do so. These periodic follow-up contacts were to occur at 2, 4, 8, 12, and 16 weeks following the assessment interview. ES staff were expected to notify UI when a claimant did not report for services.

Claimants in the second treatment group--JSA plus training or relocation--were also informed about the resource center and their obligation to maintain contact during their job search. In addition, they were told about the availability of classroom and on-the-job training and were encouraged to pursue training if interested. Staff from the local JTPA Service Delivery Area (SDA) program operator worked directly with these claimants to develop the training options. These claimants were also told about the availability of relocation assistance, which could be used for out-of-area job search and moving expenses by those who elected not to pursue training.

Claimants in the third treatment group--JSA plus a reemployment bonus--were offered the same set of JSA services as the first treatment group, in addition to a bonus for rapid reemployment. The maximum bonus equaled one-half of the claimant's remaining UI entitlement at the time of the assessment interview. This amount was available to the claimant if he or she started working either during the assessment week or in the next two weeks. Thereafter, the potential bonus declined at a rate of 10 percent of the original amount per week, until it was no longer available. Claimants recalled by their former employer could not receive a bonus; neither could those who were employed by a relative or in temporary, seasonal, or part-time jobs. Claimants who received a bonus received

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60 percent of the bonus if they were employed for 4 weeks, and the remainder if they were employed for 12 weeks.

Each of these treatments tested a different concept of the employment problems displaced workers face. The JSA-only treatment was based on the assumption that many displaced workers have marketable skills but do not have enough job-search experience to identify these skills and sell them in the job market. In contrast, the training treatment was based on the assumption that some workers' skills are outmoded and must be upgraded. Finally, the reemployment bonus treatment was based on the assumption that JSA alone is an insufficient incentive for claimants to obtain employment rapidly, and that an additional incentive will help them recognize the realities of the job market and accept a suitable job more quickly.

With the exception of the reemployment bonus and relocation assistance, the demonstration services were similar to those available under the existing ES and JTPA systems in New Jersey. However, the likelihood that a claimant was offered and received demonstration services was considerably greater than that under the existing system. Moreover, the timing of service receipt also differed: demonstration services were generally provided earlier in the unemployment spell.[1]

3. The Provision of Demonstration Services

An important objective of the demonstration was to examine how a reemployment program targeted toward UI claimants should be implemented. The demonstration design emphasized two aspects of this objective: (1) using existing agencies and vendors to provide services; and (2) using a computer-based participant tracking system to facilitate service delivery.

In the NJUIRDP, the UI agency, ES, and local JTPA program operators were all involved in delivering services, and strengthening linkages among these agencies was an important component of the demonstration. UI staff were responsible for collecting the data used to select eligible claimants and for monitoring claimants' compliance with the demonstration's reporting requirements.

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[1]See Corson et al. (1989) for further discussion.

A determination of UI eligibility was made after claimants did not report for the initial mandatory services, and, if appropriate, benefits were denied.

A four-person team in each demonstration office provided the initial reemployment services, together with additional services offered at the assessment/counseling interview. This team consisted of three ES staff members and a JTPA staff member from the local SDA program operator. An ES counselor served as team leader and had overall responsibility for ensuring that services were provided. ES staff provided all services for the JSA-only and JSA plus reemployment bonus treatment group members. JTPA staff members were involved only with the JSA plus training/relocation treatment group members. They were expected to be involved with claimants during the assessment/counseling interview and to work with individuals who were interested in classroom or on-the-job training, to identify appropriate opportunities and place claimants in them. Because the goal was to use training opportunities available in each local JTPA SDA, this component of the demonstration strengthened linkages between the ES and the local JTPA program operators in the 10 demonstration sites.

A computer-based tracking system was used extensively to operate the program and to provide some of the data used for the evaluation. Data on service delivery were entered into the system, and local office staff received lists of claimants each week who were expected to receive services. A list of claimants who did not report for services was also generated for UI, and monitoring reports were sent to central office staff. The system helped ensure that services were delivered as specified, and that claimants were not "lost" from the program.

4. The Economic Environment

During the demonstration period, the New Jersey economy experienced worker displacement caused by a long-term secular decline in manufacturing, although substantial growth occurred in other sectors. Overall, the state economy was quite strong, and the unemployment rate during the demonstration period was low (5 percent). The unemployment rate continued to be low (5 percent

or less) during the first several years of the follow-up period, but with the onset of the recent recession it rose in the last two to three years of the follow-up period to rates that ranged from about 6.6 to 8.4 percent, on an annual basis. During this later period, unemployment compensation benefits were also extended. This extension is likely to have had an effect on UI benefit receipt. As a result it could have affected our impact estimates for this period.

B. SUMMARY OF THE INITIAL AND FIRST FOLLOW-UP EVALUATION FINDINGS

The initial demonstration evaluation determined that the demonstration eligibility screens directed demonstration services to about one-quarter of the UI claimant population (Corson et al. 1989). The most important screen was the tenure requirement, which excluded individuals who had not worked for their pre-UI employer for at least three years. Other important requirements excluded individuals under age 25 and those with a definite recall date. The net result of applying the eligibility screens was an eligible population that contained a substantial proportion of older individuals, individuals whose prior job was in a declining industry, and individuals with other characteristics usually associated with the displaced worker population and with difficulties in becoming reemployed. Moreover, compared with a sample of individuals who were not eligible for the demonstration, the eligible population experienced considerably longer periods of UI collection and longer unemployment spells, on average. Thus, the eligibility screens appear to have directed demonstration services toward a population that generally faced reemployment difficulties during the year after their initial layoff.

The initial evaluation also found that the demonstration achieved its objectives of providing an increased level of reemployment services to eligible claimants and of providing these services early in the unemployment spell. Three-quarters of the claimants in the treatment groups attended the initial orientation, and three-quarters of this group continued through the initial set of job-search services to the assessment/counseling interview. The level at which demonstration-eligible claimants

received these services was substantially higher than the level at which individuals in the control group received them from the existing service network.

The evaluation showed that the demonstration was generally successful in maintaining ongoing contact with treatment group members after they received the initial set of services. The rate of training receipt for members of the second treatment group (JSA plus training or relocation) was also higher than rates for comparable groups of claimants whose exposure to training opportunities came through the regular JTPA service environment in New Jersey. However, the rate of training receipt (15 percent of those offered training) was low in absolute terms, a situation that affects our ability to detect training impacts.[2] About 19 percent of the claimants who were offered the reemployment bonus received it.

In general, the demonstration treatments were expected to hasten reemployment, thereby reducing the amount of UI collected. The potential exception was the JSA plus training or relocation treatment. Short-run UI impacts were expected to be lower for this treatment than for the others, because individuals would be eligible to continue to collect benefits while they trained. Estimates of the impacts of the treatments on UI receipt showed that all three treatments reduced the amount of benefits collected over the initial benefit year, by .47 weeks per claimant for the first treatment, .48 weeks for the second, and .97 weeks for the third. Estimates from the first follow-up study also showed further reductions in UI receipt in the second year after layoff (Anderson et al. 1991). The reductions (.53 weeks for the first treatment and .44 weeks for the third) were statistically significant. These findings suggest that all of the treatments were successful in reducing the time spent on UI, that the bonus offer provided an extra incentive to become reemployed quickly, and that the JSA component had long-run as well as short-run impacts.

Evidence on the impacts of the treatments on employment and earnings indicates that all three treatments increased employment and earnings in the year following the initial UI claim but not in

[2]Few individuals received relocation assistance.

subsequent years. These increases were larger in the first two quarters after the claim filing date than in the following two quarters, and larger for the JSA-only and JSA plus reemployment bonus treatments (relative to the JSA plus training treatment). Overall, these increases appear primarily attributable to the promotion of early reemployment through JSA. This early reemployment did not involve any sacrifice in wages. In fact, treatment group members had slightly higher hourly wages for post-UI jobs than did control group members.

Participation in training was expected to increase the trainees' long-run earnings, yet comparisons of the earnings impacts for the JSA plus training or relocation treatment with those for the JSA-only treatment suggest that the training component had no additional impact. However, only a relatively small number of claimants participated in training, so training impacts would have to be quite large to be detected. Thus, we examined trainees' earnings experiences directly to determine whether earnings patterns suggested that training may have had an impact not detected in the formal analysis. This analysis suggests that both classroom (occupational skills) and on-the-job training did enhance trainees' earnings.

The benefit-cost analysis indicates that, relative to existing services, all three treatments offered net benefits to claimants and to society as a whole. The JSA-only treatment and the JSA plus reemployment bonus treatment also led to net gains for the government sector as a whole and for the Labor Department agencies involved in the demonstration. The JSA plus training or relocation treatment was expensive for the government sector.

C. THE PURPOSE AND DESIGN OF THE SECOND FOLLOW-UP STUDY

This second follow-up evaluation of the NJUIRDP extends the analysis of demonstration impacts by approximately six years, compared with the approximately three-year period covered by the initial and first follow-up evaluations. This long-run analysis is important for the analysis of training impacts, which were expected to occur over a relatively long period. This second follow-up also provides an opportunity to examine long-run earnings patterns for displaced workers, to determine whether the

method used in the demonstration to identify displaced workers was successful in identifying workers with long-run employment difficulties. Alternative methods of identifying displaced workers are also investigated.

To examine impacts on UI receipt, we collected administrative data on all new, initial claims (benefit years) established after the claim that made individuals eligible for NJUIRDP.[3] New claims through September 1993 and all payments made as of mid-October 1993 were included. We used these data to construct variables describing UI activity by year, with the years defined according to the initial claim date making the individual eligible for NJUIRDP. For example, the year of the initial claim, or first year, for an individual with an initial claim date in July 1986 was the period from July 1986 through June 1987, the second year was from July 1987 through June 1988, and so on. The variables that describe UI activity used in our analysis are (1) whether a claim was established in the year, and (2) benefits and weeks of UI collected on this claim. Thus, the benefits and weeks collected on a claim established in, say, the second year, could have been received in the subsequent year. We report estimates of UI receipt for six years that include the initial claim year, although the measures of weeks and dollars collected are truncated for the last year and are thus underestimates.[4]

Data on weeks worked and earnings were obtained from quarterly wage records through the second quarter of 1993, to examine impacts on employment and earnings. These records include earnings on all UI-covered jobs in New Jersey, but they exclude earnings obtained outside the state and earnings in uncovered employment, such as self-employment. For this reason, the wage-records earnings are underestimates. Treatment impacts are also likely to be underestimated, because they were not measured for out-of-state or uncovered earnings.

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[3]Data for the first several years were collected for the first follow-up study; the remaining data were collected for the second follow-up study.

[4]The sixth-year data are truncated because full-benefit-year data are unavailable for claims established after mid-October 1992. Because initial enrollment in the demonstration occurred during July 1986 to June 1987, some initial claims that occurred in the sixth year following enrollment could have occurred after mid-October 1992. Benefit years for these claims ended after October 1993.

For the analysis, data on quarterly earnings and weeks worked on all jobs were totaled by calendar quarters and years (defined relative to the initial date of the claim). In all, we report data for six years, beginning with the first calendar quarter and year after the initial claim date.

D. OUTLINE OF THE REPORT

The remainder of this report contains four chapters. Chapter II presents our estimates of the demonstration impacts on UI receipt for the three treatment groups, for six years that include the initial benefit year. It also presents impacts on earnings and weeks worked for the three treatment groups during the six years after the initial claim date. Chapter III examines the experience of individuals who received training. Chapter IV discusses strategies for targeting services to claimants who appear to need and can benefit from them. Chapter V updates the benefit-cost analysis to consider the impacts during the entire follow-up period.

II. IMPACTS ON UI RECEIPT AND EARNINGS

The initial evaluation of the New Jersey Unemployment Insurance Reemployment Demonstration Project (NJUIRDP) showed that each treatment led to a statistically significant reduction in Unemployment Insurance (UI) receipt during the initial benefit year, as expected. This reduction was reflected in both the amount of benefits and the number of weeks collected. The first follow-up evaluation also showed reductions in UI receipt in the year after the initial benefit year. These impacts were statistically significant for two of the treatments: (1) the job-search assistance (JSA) only; and (2) the JSA plus reemployment bonus. These evaluations also found that all three treatments increased employment and earnings in the year after the initial UI claim, but no significant impacts on employment and earnings were observed for subsequent years.

This chapter extends the analysis of longer-term effects of the treatments for a six-year period, based on UI administrative records and quarterly wage records for the treatment and control samples. It shows that the JSA plus reemployment bonus treatment led to further reductions in regular UI receipt two years after the initial benefit year. It also suggests that the JSA plus training or relocation treatment led to a reduction in benefit receipt under the recent Emergency Unemployment Compensation (EUC) program. While no statistically significant long-term effects on employment and earnings were found, the modest impacts on earnings suggested by the impacts on UI receipt could still have occurred.

In combination with findings from earlier studies, these findings suggest that each treatment reduced the amount of UI benefits received in the initial benefit year and in subsequent years. Moreover, the findings suggest that all of the treatment components--JSA, training, and the reemployment bonus--may have contributed to these longer-term impacts. The long term impacts on UI receipt suggest that the treatments generated jobs that were more stable than those found by control group members.

A. IMPACTS ON UI RECEIPT

The demonstration treatments were expected to and did reduce the amount of UI collected in the initial benefit year. Somewhat unexpectedly, the first follow-up study showed that reductions in UI receipt also occurred in the year after the initial benefit year. These reductions were statistically significant for two treatments (JSA only and JSA plus a reemployment bonus) and similar in magnitude, suggesting that the JSA component led to the longer-term impacts. These longer-term impacts also suggest that the treatments led to jobs that were more stable than those found by control group members.

To investigate the impacts of the treatments over a six-year period, we obtained UI administrative records for the treatment and control samples that covered the period from the initial benefit year through mid-October 1993. Sample members entered the demonstration from July 1986 to June 1987, so this time frame provides data for the initial claim year (the first year) and five subsequent years (the second through sixth years). The data for the sixth year are limited because complete benefit histories are not available for all claimants who began collecting benefits in the sixth year. However, the application of random assignment procedures in the demonstration means that all treatment and control groups are affected equally by the incomplete sixth-year data, so we have chosen to report the data. Nevertheless, because the six-year data are incomplete, our estimates of the mean levels of UI receipt and our estimates of treatment impacts are biased downward for that year.[1]

To estimate the long-term effects of the treatments, we examined UI receipt for the entire follow-up period and by year. The initial benefit year, or first year, was the 364 days beginning with the initial date of the claim, the second year was defined as the next 364 days, and so on.[2] Impacts

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[1]In addition, we have no information on any UI received by sample members from states other than New Jersey. As with the sixth-year data, our estimates of UI receipt and treatment impacts are probably biased downward.

[2]The New Jersey UI benefit year is 364 days (52 weeks).

were estimated with regressions that controlled for the quarter of enrollment in the demonstration; gender, race/ethnicity, and age; base period earnings; industry; use of a union hiring hall; expectation of recall; potential UI duration; weekly benefit amount; and local office.[3] The tables that report the results show the impacts on treatment group members and the control group means. Treatment group means may be estimated by adding the impact to the control mean.

Tables II.1 and II.2 show, for regular UI, estimated impacts of the treatments on the amount of benefits received and the number of weeks of payments. These impacts are consistent with those reported in the previous NJUIRDP evaluations. The only difference is that the impact estimate in the third year for the JSA plus reemployment bonus treatment is slightly larger than the estimate reported in the first follow-up and is now statistically significant.[4] This finding suggests that the reemployment bonus component of the demonstration, as well as the JSA component, contributed to the longer-run UI impacts.

A further change from the first follow-up study is that the estimated reductions in regular UI receipt for the full follow-up period (now six years) are statistically significant for only one treatment (the JSA plus reemployment bonus treatment). This situation occurs because the longer follow-up period introduces random components into the estimates, by adding data for three years (the fourth through sixth) in which no impacts occurred. Nevertheless, the point estimates for the entire follow-up period show substantial reductions in regular UI receipt for all treatments.

Estimates of the impacts on the yearly probability of collecting UI (Table II.3) show that the reduction in UI benefits was achieved primarily through a reduction in the probability of receiving regular UI. More specifically, the JSA only and JSA plus reemployment bonus treatments led to a statistically significant reduction in the probability of receiving regular UI in the second year. A statistically significant reduction in the probability of receiving regular UI was also found for the third

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[3]These variables were defined as of the date of enrollment in the demonstration.

[4]Data for the third year were incomplete at the time of the first follow-up, which accounts for the change in the estimate.

TABLE II.1

IMPACTS OF THE TREATMENTS ON UI DOLLARS RECEIVED
(Standard Error in Parentheses)

	JSA Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus	Control Group Mean
Year of Initial Claim (First Year)	-87 * (46)	-81 ** (41)	-170 *** (45)	3,228
Second Year	-94 *** (36)	-39 (33)	-78 ** (36)	600
Third Year	-13 (39)	-15 (35)	-65 * (39)	560
Fourth Year	9 (43)	-22 (39)	-6 (43)	569
Fifth Year	-17 (47)	-23 (42)	36 (47)	588
Sixth Year	-13 (43)	15 (39)	-52 (43)	486
Total	**-181 (132)**	**-165 (119)**	**-333 ** (131)**	**6,031**

NOTE: The sample used for this analysis includes 2,416 JSA only sample members, 3,810 JSA plus training or relocation sample members, 2,449 JSA plus reemployment bonus sample members, and 2,385 control group members. The estimates are regression-adjusted treatment-control differences.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE II.2

IMPACTS OF THE TREATMENTS ON UI WEEKS PAID
(Standard Error in Parentheses)

	JSA Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus	Control Group Mean
Year of Initial Claim (First Year)	-0.47 ** (0.24)	-0.48 ** (0.22)	-0.97 *** (0.24)	17.9
Second Year	-0.53 *** (0.19)	-0.22 (0.17)	-0.44 ** (0.19)	3.3
Third Year	.008 (.19)	-.09 (.18)	-.31 (.19)	3.0
Fourth Year	.19 (.20)	-.09 (.18)	.05 (.20)	2.8
Fifth Year	.08 (.20)	-.10 (.18)	.16 (.20)	2.7
Sixth Year	-.03 (.18)	.06 (.16)	-.21 (.18)	2.2
Total	**-.76 (.64)**	**-.93 (.58)**	**-1.72 *** (.64)**	**31.9**

NOTE: The sample used for this analysis includes 2,416 JSA only sample members, 3,810 JSA plus training or relocation sample members, 2,449 JSA plus reemployment bonus sample members, and 2,385 control group members. The estimates are regression-adjusted treatment-control differences.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE II.3

IMPACTS OF THE TREATMENTS ON THE PROBABILITY OF UI RECEIPT
(Standard Error in Parentheses)

Probability of Receipt	JSA Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus	Control Group Mean
Second Year	-.021 *	-.011	-.019 *	.219
	(0.11)	(.010)	(.011)	
Third Year	-.007	-.008	-.019 *	.183
	(.010)	(.009)	(.010)	
Fourth Year	.000	-.006	-.009	.165
	(.010)	(.009)	(.010)	
Fifth Year	.006	-.007	.002	.151
	(.010)	(.009)	(.010)	
Sixth Year	-.006	.000	-.014	.122
	(.009)	(.008)	(.009)	
Total Number of Claims After Initial Claim (Year 2 to Year 6)	**-.027**	**-.031**	**-.059 ***	**.840**
	(.033)	**(.030)**	**(.033)**	

NOTE: The sample used for this analysis includes 2,416 JSA only sample members, 3,810 JSA plus training or relocation sample members, 2,449 JSA plus reemployment bonus sample members, and 2,385 control group members. The estimates are regression-adjusted treatment-control differences.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

year for the JSA plus reemployment bonus group. When compared with the estimates for the reduction in regular UI benefits, the reduction in the probability of receiving regular UI accounts for about two-thirds of the reduction in regular UI receipt in the second year and all of the reduction in the third year. Finally, the number of claims over the entire follow-up period also declined for each treatment, but the impact was statistically significant only for the JSA plus reemployment bonus.

The second and third year impacts on UI receipt must arise through reductions in UI benefit receipt among claimants who, in the absence of the demonstration treatments, would collect benefits each year. Thus it is useful to examine the characteristics of control group members who collect benefits in multiple years. When we do this by examining the characteristics of control group members who collect UI in the second year, we find that the major difference between those collecting benefits in the second year and those not collecting benefits is that those collecting benefits were considerably more likely to have been on indefinite layoff (that is, have expected to be recalled but did not have a definite recall date) at the time of the initial layoff that made them eligible for the demonstration. Specifically, 65 percent of those collecting benefits in the second year were on indefinite layoff as compared to 27 percent of those not collecting. This finding suggests that the reductions in UI receipt in the second year would probably be substantially less if the eligible population excluded claimants on indefinite layoff.

Toward the end of the follow-up period, several extended benefits programs were available in New Jersey. These programs included New Jersey Emergency Unemployment Benefits (EUB), in effect from August 19, 1991 to November 16, 1991, prior to passage of the federal Emergency Unemployment Compensation (EUC) Program. The EUC program began on November 17, 1991, and continued throughout the remainder of our follow-up period. EUC claims filed by NJUIRDP sample members began in the demonstration's fifth or sixth follow-up year. A final state extended benefits program--Additional Benefits for Training (ABT)--was available beginning in October 1992

for claimants who entered approved training. This extended benefits program is part of the state's Workforce Development Partnership Program.

On average, demonstration claimants collected very few benefits under the two special state programs (see Tables II.4 and II.5) but collected substantially more under the EUC program. For example, control group members collected, on average, $763 under the EUC program, which is about 70 percent of the average collected under regular UI during the same time period (the fifth and sixth years).

More important, our estimates show an impact of the JSA plus training or relocation assistance treatment on benefit collection under the EUC program. This impact estimate (a reduction in EUC benefits of $125 or .53 weeks) is surprising, because no impacts on regular UI receipt were observed for any treatment during the period EUC was in effect (that is, during the fifth and sixth follow-up years). Although this estimate could be a statistical anomaly, it could also represent a true impact, because the JSA plus training or relocation assistance treatment is expected to have long-run impacts. Given this uncertainty, we view this impact estimate with caution.

Each NJUIRDP treatment appears to have reduced the amount of UI benefits received both in the initial benefit year and in subsequent years. Statistically significant reductions in UI benefits occurred in the year after the initial benefit year for the JSA only and JSA plus reemployment bonus treatments, in the next year for the JSA plus reemployment bonus treatment, and in the recent EUC program for the JSA plus training or relocation assistance treatment. Over all UI programs (see Table II.5), the NJUIRDP treatments appear to have reduced UI benefit receipt by about three-quarters of a week for the JSA only treatment, by one and a half weeks for the JSA plus training or relocation assistance treatment, and by nearly two weeks for the JSA plus reemployment bonus treatment. These findings suggest each of the treatment components--JSA, training, and the reemployment bonus--probably contributed to the longer-term impacts and that the treatments, in general, generated jobs that were more stable than those found by control group members. This

TABLE II.4

IMPACTS OF THE TREATMENTS ON TOTAL UI DOLLARS RECEIVED
THROUGH OCTOBER 1993, BY PROGRAM
(Standard Errors in Parentheses)

Extended Benefits Program	JSA Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus	Control Group Mean
EUB	-4	-3	2	48
	(7)	(6)	(7)	
EUC[a]	-40	-125 **	-46	763
	(58)	(52)	(57)	
ABT[a]	2.7	-.9	2.1	10
	(6.6)	(6.0)	(6.6)	
Regular UI[b]	-181	-165	-333 *	6,031
	(132)	(119)	(131)	
All UI[a]	-222	-293 *	-375 **	6,852
	(171)	(155)	(170)	

NOTE: The sample used for this analysis includes 2,416 JSA only sample members, 3,810 JSA plus training or relocation sample members, 2,449 JSA plus reemployment bonus sample members, and 2,385 control group members. The estimates are regression-adjusted treatment-control differences.

[a]For some sample members, we did not have complete data on these variables for the sixth year following random assignment.

[b]Regular UI impacts are for the first through sixth year after random assignment.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE II.5

IMPACTS OF THE TREATMENTS ON TOTAL UI WEEKS PAID
THROUGH OCTOBER 1993, BY PROGRAM
(Standard Errors in Parentheses)

Extended Benefits Program	JSA Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus	Control Group Mean
EUB	.00 (.03)	.00 (.03)	.01 (.03)	.24
EUC[a]	-.03 (.26)	-.53 ** (.23)	-.22 (.26)	3.52
ABT[a]	.01 (.03)	.00 (.02)	.01 (.03)	.04
Regular UI[b]	-.76 (.64)	-.93 (.58)	-1.72 *** (.64)	31.85
All UI[a]	-.78 (.80)	-1.47 ** (.73)	-1.92 ** (.80)	35.66

NOTE: The sample used for this analysis includes 2,416 JSA only sample members, 3,810 JSA plus training or relocation sample members, 2,449 JSA plus reemployment bonus sample members, and 2,385 control group members. The estimates are regression-adjusted treatment-control differences.

[a]For some sample members, we did not have complete data on these variables for the sixth year following random assignment.

[b]Regular UI impacts are for the first through sixth year after random assignment.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

finding differs from the first follow-up finding, which attributed longer-run impacts solely to the JSA component of the treatments.

B. IMPACTS ON EARNINGS

The initial and first follow-up evaluations of the demonstration showed that each of the treatments increased earnings in the year of the initial UI claim, and that these earnings increases were concentrated in the first two quarters immediately following the initial claim. The earnings impacts were also found to be lowest for the JSA plus training or relocation treatment during this period. This result was expected, because participation in training reduced the time available for employment. Any training impacts on earnings were expected to occur over a longer period, after training was completed.

These results are based on employment and earnings data collected from a survey of sample members, which permitted a detailed investigation of the timing of the impacts. In particular, this analysis focused on quarterly earnings, with the quarters defined relative to the initial UI claim date. An additional analysis was also performed on the basis of quarterly wage records. Because these data are collected on a calendar basis, this analysis could focus only on calendar quarters that began after the claim date. The analysis found that the JSA plus reemployment bonus treatment had a statistically significant impact on earnings in the first quarter following the claim, but that the impacts of the other treatments, while positive, were not statistically significant.[5]

To investigate whether the treatments led to longer-run impacts, we collected quarterly wage records through the second quarter of 1993. These records provided six years of earnings and weeks

[5]The difference in the findings from the two data sources could arise for a number of reasons, including differences in how the quarters were defined, misreporting in the survey, or the fact that wage records are available for UI-covered employment only in New Jersey. Although we have no reason to suspect that the treatments had an impact on the probability of working in covered employment in New Jersey, the unavailability of wage-records data on uncovered jobs and on jobs outside of New Jersey is likely to bias the impact estimates downward, because using wage records data involves the implicit assumption that the treatment-control difference in uncovered employment is zero.

worked data for all members of the sample, beginning with the first calendar quarter and year after the claim date. The treatment impacts were estimated for these six years in the same manner as the UI impacts were estimated--with a regression that controlled for the quarter of enrollment in the demonstration; gender, race/ethnicity, and age; base period earnings; industry; use of a union hiring hall; expectation of recall; potential duration; weekly benefit amount; and local office.

Tables II.6 to II.8, which report the results of this analysis for the probability of working, earnings, and weeks worked, show no change from the first follow-up. As shown in the tables, the only statistically significant impact for all measures of employment and earnings is the one reported earlier--that is, the JSA plus reemployment bonus treatment increased the probability of working (by .04), earnings (by $176), and weeks worked (by .37) in the first quarter following the claim date. The JSA plus reemployment bonus treatment also led to a statistically significant increase in weeks worked in the second quarter following the date of claim. While the impact estimates for the other two treatments are not statistically significant, the point estimates of the first year impacts are consistent with the statistically significant impacts on UI weeks. Our estimates of earnings impacts in Year 2 and Year 3, while insignificant and sometimes negative, are also consistent with the statistically significant impacts on UI weeks that we found for those years. This difference in the ability to detect impacts arises because the variation in earnings among individuals is considerably larger than the variation in UI weeks. Hence, the standard error of our earnings impact estimates are too large to detect the modest earnings gains we would expect, given the UI impacts.

Although there are few impacts to report, it is useful to investigate the pattern of employment and earnings over time. This investigation (using the control group) indicates that less than 50 percent of control group sample members were in covered employment in New Jersey in the first quarter after the claim (Table II.9).[6] This percentage rose substantially in the second quarter to 57

[6]It is important to remember that, because of missing wage records for those in uncovered jobs or in covered jobs outside of New Jersey, the proportion employed is biased downward.

TABLE II.6

IMPACTS OF THE TREATMENTS ON THE PROBABILITY OF WORKING[a]
(Standard Error in Parentheses)

Quarter/Year After Claim	JSA Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus	Control Group Mean
Quarter				
1	0.018	0.015	0.040 ***	0.49
	(0.014)	(0.013)	(0.014)	
2	0.021	0.002	0.022	0.57
	(0.014)	(0.012)	(0.014)	
3	0.012	0.005	0.003	0.63
	(0.014)	(0.012)	(0.013)	
4	0.005	-0.004	-0.006	0.63
	(0.014)	(0.012)	(0.014)	
Year				
1	.008	.011	.012	.76
	(.012)	(.011)	(.012)	
2	.005	-.001	-.005	.73
	(.013)	(.011)	(.012)	
3	.009	-.002	-.011	.69
	(.013)	(.012)	(.013)	
4	.019	.000	.004	.64
	(.013)	(.012)	(.013)	
5	.020	.008	.007	.59
	(.014)	(.012)	(.014)	
6	.009	.009	.000	.55
	(.014)	(.012)	(.014)	
Total (Year 1 to Year 6)	**.070 (.063)**	**.024 (.057)**	**.008 (.063)**	**3.96**

NOTE: The sample used for this analysis includes 2,416 JSA only sample members, 3,810 JSA plus training or relocation sample members, 2,449 JSA plus reemployment bonus sample members, and 2,385 control group members. The estimates are regression-adjusted treatment-control differences.

[a]The probability of working is defined as having reported earnings in a quarter or in a year.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE II.7

IMPACTS OF THE TREATMENTS ON EARNINGS, IN DOLLARS
(Standard Error in Parentheses)

Quarter/Year After Claim	JSA Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus	Control Group Mean
Quarter				
1	28	58	176 **	1,638
	(83)	(75)	(83)	
2	75	-23	79	2,174
	(90)	(81)	(89)	
3	101	47	46	2,507
	(82)	(75)	(82)	
4	31	28	79	2,517
	(86)	(77)	(85)	
Year				
1	235	109	379	8,836
	(266)	(241)	(265)	
2	279	-149	-21	11,253
	(327)	(296)	(326)	
3	143	-2	3	11,831
	(363)	(328)	(361)	
4	181	57	434	11,679
	(376)	(341)	(375)	
5	121	-67	-113	11,647
	(400)	(362)	(349)	
6	193	283	193	11,188
	(412)	(373)	(410)	
Total	**1,152**	**232**	**874**	**66,434**
(Year 1 to Year 6)	**(1,811)**	**(1,640)**	**(1,805)**	

NOTE: The sample used for this analysis includes 2,416 JSA only sample members, 3,810 JSA plus training or relocation sample members, 2,449 JSA plus reemployment bonus sample members, and 2,385 control group members. The estimates are regression-adjusted treatment-control differences.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE II.8

IMPACTS OF THE TREATMENTS ON WEEKS WORKED
(Standard Error in Parentheses)

Quarter/Year After Claim	JSA Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus	Control Group Mean
Quarter				
1	0.18	0.12	0.37 ***	
	(0.14)	(0.13)	(0.14)	3.9
2	0.21	-0.01	0.27 *	5.6
	(0.16)	(0.14)	(0.16)	
3	0.21	0.08	0.08	6.7
	(0.16)	(0.15)	(0.16)	
4	0.09	-0.02	0.05	6.6
	(0.17)	(0.15)	(0.17)	
Year				
1	.68	.17	.77	22.8
	(.51)	(.46)	(.51)	
2	.45	-.31	-.18	27.6
	(.59)	(.54)	(.59)	
3	.25	-.12	.10	26.8
	(.62)	(.56)	(61)	
4	.67	.22	.60	24.9
	(.62)	(.56)	(.62)	
5	.78	.49	.27	22.9
	(.62)	(.57)	(.62)	
6	.49	.40	.35	21.3
	(.62)	(.56)	(.62)	
Total	**3.33**	**.85**	**1.92**	**146.3**
(Year 1 to Year 6)	**(3.00)**	**(2.71)**	**(2.99)**	

NOTE: The sample used for this analysis includes 2,416 JSA only sample members, 3,810 JSA plus training or relocation sample members, 2,449 JSA plus reemployment bonus sample members, and 2,385 control group members. The estimates are regression-adjusted treatment-control differences.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE II.9

MEAN EARNINGS AND EMPLOYMENT FOR THE CONTROL GROUP

	(1)	(2)	(3)
	Probability of Working	Mean Earnings (Dollars)	Mean Earnings Minus Base Period Earnings for Employed Individuals (Dollars)
Base Period (Annual Earnings)	1.0	17,908	0
Quarter (Year 1)			
1	.49	1,638	-1,234
2	.57	2,174	-534
3	.63	2,507	-419
4	.63	2,517	-378
Year			
1	.76	8,836	-6,009
2	.73	11,253	-1,962
3	.69	11,831	-758
4	.64	11,679	72
5	.59	11,647	1,291
6	.55	11,188	1,889

NOTE: Mean earnings in column 2 are computed over the entire control group. Mean earnings in column 3 are computed in each follow-up quarter or year for individuals who were employed.

percent and to 63 percent in the third and fourth quarters. Annually, 76 percent were employed at some time during the initial benefit year. This percentage declined in each subsequent year and reached 55 percent in year six. However, this decline is probably a reflection of the fact that some claimants moved from New Jersey or withdrew from the labor force, rather than a reflection of increased unemployment (the probability of beginning a UI claim also declined each year--see Table II.3).

Because the absence of New Jersey wage records data does not necessarily mean that an individual is unemployed, we can focus on the experiences of individuals who are employed in New Jersey to gain further insights into the employment experiences of claimants. We do this by examining the pattern of post-UI quarterly earnings relative to quarterly base period earnings for individuals who were employed in covered employment.[7] This analysis shows the pattern of earnings recovery (see Table II.9). In the first quarter after the initial claim, quarterly earnings were well below those for the base period (the average difference was -$1,234), because many claimants ended their UI spell within the quarter and thus did not work the entire quarter. In the second quarter, more individuals worked the full quarter, and the difference declined. By the fourth quarter, the average difference was -$378. By this point, most individuals who were working worked the full quarter, and the negative difference indicates that, on average, claimants were in lower-paying jobs than they were prior to the initial UI claim.

Data by year show that average earnings for *employed* individuals did not reach pre-UI levels until the fourth year after the initial claim, when average earnings exceeded the base period level by $72. By the sixth year, average earnings for employed individuals exceeded the base period average by $1,889. However, this 10.5 percent increase in nominal earnings did not keep pace with inflation (the consumer price index for the Northeast rose approximately 34 percent in this period), or with

[7]The variable used for this analysis is defined as quarterly earnings minus average quarterly earnings during the base period, conditional on the presence of earnings in the quarter.

the average weekly earnings of manufacturing workers in New Jersey (average weekly earnings rose by approximately 25 percent in this period).[8]

Information on employer attachment, reported in Table II.10, provides a further measure of long run employment outcomes among control group members. These data show that, in the six years following their layoff, the majority of the claimants targeted by the NJUIRDP had either no attachment or very little attachment to the base period employer who paid them the largest amount of wages.[9] Two-thirds received no earnings from the largest base period employer and an additional 9 percent received earnings in only one quarter (comparisons of interview and wage record data suggest that, in many cases, these reported earnings may have been severance payments).[10] The remaining claimants had 2 or more quarters with earnings, but, even among this group, very few had earnings from the largest base period employer throughout the follow-up period. Only 7 percent had earnings from the largest base period employer for more than four years following the initial layoff.

In summary, our wage-records-based analysis of employment and earnings following the initial UI claim suggests that at least one treatment, the JSA plus reemployment bonus, increased earnings initially. None of the treatments had statistically significant longer-run impacts on the probability of working, the amount of earnings, or weeks worked. However, since the variation in earnings among claimants is quite large the modest impacts suggested by the UI impact estimates could still have occurred.

Finally, our examination of earnings of employed control group members shows that nominal annual earnings remained below base period levels until the fourth year after the initial UI claim and

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[8]These comparisons were made from April 1986, the midpoint of the average individual's base period, to July 1992, the midpoint of the sixth year after the initial claim, for the average sample member.

[9]An alternative and perhaps preferable way of examining employer attachment would be to examine attachment to the employer whose layoff led to the UI claim. However, because data identifying the layoff employer were not available for all members of the sample, we chose to use the largest layoff employers. This approach provides consistent data on the entire sample.

[10]See Appendix D in Corson et al., 1992.

TABLE II.10

ATTACHMENT TO THE LARGEST BASE PERIOD EMPLOYER

Number of Post UI Quarters with Earnings from Largest Base Period Employer	Percent
0	67.0
1	9.2
2-4	5.9
5-8	5.6
9-12	3.7
13-16	2.2
17-20	2.6
21-24	3.9

NOTE: The distribution shows the number of quarters in the first six years after the UI claim in which earnings from the largest base period employer are reported in wage records.

that, even by the sixth year, earnings of employed individuals had not kept pace with inflation. These findings suggest that, on average, claimants were unable to obtain reemployment in jobs with the same earnings potential as that of their pre-UI jobs.

III. UI RECEIPT AND EARNINGS FOR THOSE RECEIVING TRAINING

This chapter extends the analysis in the previous chapter by examining Unemployment Insurance (UI) receipt and employment and earnings for individuals who received training. This group is of interest because of the expectation that the training they received would help them increase their future earnings. The analysis of differences among treatment groups in the previous chapter did not find such impacts. However, the analysis presented here suggests that classroom and on-the-job training did in fact enhance the economic position of claimants who participated.

In Chapter II, we found that the impacts of the job-search assistance (JSA) plus training or relocation treatment on UI and earnings were not significantly greater than the impacts of the JSA-only treatment. However, this conclusion may be misleading. Only a small percentage (15 percent) of individuals who were offered training actually received it, so training impacts would need to be quite large (on the order of $1,500 a quarter) to be detected.[1] Impacts of this magnitude are much larger than have typically been found in evaluations of training programs (for a summary, see Leigh 1990). In this chapter, we examine trainees' UI and earnings experience to determine whether training may have had an impact that was not detected in the treatment to treatment group comparison.

For the analysis presented in this chapter, we compared the UI and earnings experience of trainees with the experience of assessed JSA-only claimants. Because the training offer was made at the assessment interviews, we could create a sample of JSA-only claimants who were more closely comparable to the trainees by excluding JSA-only claimants who were not assessed. Despite this effort to create the most appropriate comparison group, we could not interpret the benefit and

[1]For example, if we compared the quarterly earnings of claimants in the JSA-only treatment with the quarterly earnings of those in the JSA plus training or relocation treatment, the training impacts would need to be at least $1,500 per quarter, per trainee, to be detected with a 70 percent chance under a one-tail test at a 95 percent confidence level.

earnings differences between the trainees and the assessed JSA-only group as estimates of the impact of training, because training participants were not chosen at random. Individuals who participated in training were likely to differ systematically from those who were offered training but chose not to participate. However, we had no equivalent group of nontrainees with which to compare the training participants, as a basis for estimating the impact of training.

We extended this analysis by controlling for the observed differences between the trainees and nontrainees, using regression methods for estimating the effect of training on employment and UI receipt. The regression specification for this analysis included as the dependent variable the change in earnings between the base period and the postclaim quarter. Hence, we measured the effect of training as the estimated impact on the relative change in earnings between the base period and the postclaim period.[2] These results may also provide biased estimates of the effects of training, to the extent that *unobserved* factors affect both the self-selection of training participants and workers' earnings and UI receipt outcomes.

These analyses showed that the labor market and UI benefit experience of claimants in the JSA plus training or relocation treatment who received training appeared to differ significantly from the experience of claimants who were assigned to the JSA-only group and were assessed. Claimants who received on-the-job training experienced relatively high levels of employment and earnings throughout the period following the initial claim. These claimants also received relatively lower levels of UI benefits than the assessed JSA-only claimants following the initial claim. On the other hand, claimants who received classroom training (which involved occupational training, as opposed to remedial or general education) experienced less employment and lower earnings than the JSA-only claimants in the first three quarters following the claim date. In subsequent quarters, the classroom

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[2]Ashenfelter (1978) and Card and Sullivan (1988) show that a similar estimator yields a consistent estimate of the impact on earnings if (1) shocks in earnings are uncorrelated with their own lagged values and with the decision to participate in training, and (2) the individual-specific component of earnings enters linearly into the earnings equation. The implications of these assumptions are discussed in Appendix A of Anderson et al. (1991).

trainees experienced employment and earnings that were somewhat higher than the employment and earnings of the assessed JSA-only group. The pattern of UI receipt among classroom trainees was consistent with their employment and earnings experience. The classroom trainees received relatively high benefits in the year of their initial claim, but relatively low benefits in subsequent years. The patterns of earnings and UI receipt did not change appreciably when we controlled for individuals' characteristics. These findings suggest that training enhanced the employment and earnings of both on-the-job trainees and classroom trainees in the long run.

A. EARNINGS AND WEEKS WORKED FOR TRAINEES

The 314 claimants in the JSA plus training or relocation treatment who participated in classroom training received significantly lower earnings, on average, than the assessed JSA-only group members in the first three quarters following the claim date (see Table III.1). However, these differences are not surprising, given that many individuals did not work while they were attending classroom training.

After the third quarter following the claim date, earnings for the classroom trainees were higher than earnings for the assessed JSA-only group. These differences increased and reached a peak during the third year of followup, when classroom trainees earned approximately $1,300 more than the assessed group. In subsequent years, this difference declined somewhat and was no longer statistically significant. As noted, these differences should not be interpreted as impacts of training because the classroom trainees and assessed JSA-only groups were drawn from two different populations. The individual characteristics of these two groups differed at the point of random assignment, so one would expect that their subsequent earnings would be different.[3]

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[3]Relative to the JSA-only assessed group, both classroom and on-the-job trainees were more likely to be black and age 34 or younger. In addition, relatively few trainees expected to be recalled. Finally, classroom trainees were more likely to be women than were those in the JSA-only assessed group. We controlled for all of these differences in the impact regressions reported in the text below and in Tables III.2, III.3, III.5, and III.6.

TABLE III.1

AVERAGE EARNINGS OF TRAINING RECIPIENTS, IN DOLLARS
(Standard Deviations in Parentheses)

Period	JSA Plus Training or Relocation: Classroom Trainees	JSA Plus Training or Relocation: On-the-Job Trainees	JSA-Only: Assessed Claimants
Quarter			
Base Period (Average per Quarter)	4,662 (2,382)	4,085 (2,345)	4,735 (2,999)
1	610 *** (1,624)	1,926 ** (2,454)	1,109 (2,805)
2	1,001 *** (2,818)	3,375 *** (2,520)	1,682 (3,314)
3	1,868 ** (2,371)	4,220 *** (3,058)	2,230 (2,858)
4	2,442 (2,616)	4,675 *** (3,640)	2,299 (2,983)
Year			
1	5,920 *** (6,420)	14,196 *** (10,464)	7,320 (8,925)
2	11,601 * (10,257)	18,895 *** (13,554)	10,422 (11,907)
3	12,678 * (11,872)	23,220 *** (21,550)	11,346 (13,053)
4	12,444 (12,340)	20,073 *** (13,935)	11,363 (13,524)
5	12,184 (13,003)	19,232 *** (15,711)	11,379 (14,770)
6	11,996 (13,613)	20,682 *** (17,710)	11,214 (15,254)
Total (Years 1 to 6)	66,825 (58,091)	116,299 *** (79,793)	63,043 (66,459)
Number of Observations	**314**	**45**	**1,363**

*Significantly different from the JSA-only treatment mean at the 90 percent confidence level in a two-tail test.

** Significantly different from the JSA-only treatment mean at the 95 percent confidence level in a two-tail test.

*** Significantly different from the JSA-only treatment mean at the 99 percent confidence level in a two-tail test.

However, even when we controlled for differences in individual characteristics, our impact estimates suggested that classroom training reduced earnings in the first two quarters after the initial claim, and then enhanced the earnings of the trainees in later periods (see Table III.2).[4]

We obtained similar findings for the impacts of training on average weeks worked (see Table III.3).[5] Classroom training had a negative impact on weeks worked during the first three quarters and a positive impact on weeks worked during subsequent quarters. On-the-job training had a positive impact on weeks worked during every quarter of followup.

A relatively small number of claimants in the JSA plus training or relocation treatment (45 individuals) received on-the-job training. Claimants who received on-the-job training had significantly higher earnings than did the assessed JSA-only claimants in all quarters following the first quarter after the claim date, as shown in Table III.1. To some extent, this result is not surprising, because, by definition, on-the-job training recipients should have been employed, at least in the early quarters. However, the higher earnings of on-the-job trainees persisted because the trainees remained employed and their earnings grew over time. By the third year of followup, on-the-job trainees were receiving over 42 percent more earnings than they received during the four quarters of the base period. On-the-job trainees replaced their base-period earnings to a much greater extent than did the assessed JSA-only claimants, who received third-year earnings that were 40 percent lower than their base-period earnings. This evidence cannot be used to argue that on-the-job training will increase earnings for a randomly chosen group of UI claimants, but it does demonstrate that the claimants who received on-the-job training achieved a relatively high level of earnings after the demonstration.

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[4]As noted, the dependent variable used in the earnings impact regression was the change in earnings between the base period and the postclaim quarter.

[5]For these impact regressions, we used a dependent variable equal to the number of weeks worked in a given period.

TABLE III.2

ESTIMATED IMPACTS OF TRAINING ON THE AVERAGE QUARTERLY EARNINGS OF TRAINING RECIPIENTS, IN DOLLARS
(F Statistics in Parentheses)

	Classroom Training	On-the-Job Training
Quarter		
1	-458 **	1,469 ***
	(4.96)	(8.67)
2	-635 ***	2,347 ***
	(7.05)	(16.33)
3	-314	2,632 ***
	(2.20)	(26.21)
4	195	2,995 ***
	(0.76)	(30.32)
Year		
1	-1,212 *	9,443 ***
	(2.73)	(28.11)
2	1,402	10,987 ***
	(2.53)	(26.33)
3	1,561 *	14,387 ***
	(2.86)	(41.22)
4	1,298	11,232 ***
	(1.85)	(23.59)
5	1,025	10,357 ***
	(1.00)	(17.27)
6	1,004	11,954 ***
	(.95)	(22.98)

NOTE: The estimated impacts of training are based on regressions that include training indicators and a set of interaction terms, where the interaction terms are equal to the product of the training indicators and a variety of economic and demographic variables. The hypothesis test used to evaluate the statistical significance of the estimated impact is an F test of the linear equation implied by having the training indicators take a value of 1. The test is calculated according to the assumption that the economic and demographic variables are equal to the means for the training groups. The critical values for the F statistic are 2.71, 3.84, and 6.63 for the 90, 95, and 99 percent confidence levels, respectively.

* Significantly different from zero at the 90 percent confidence level in an F test.
** Significantly different from zero at the 95 percent confidence level in an F test.
***Significantly different from zero at the 99 percent confidence level in an F test.

TABLE III.3

ESTIMATED IMPACTS OF TRAINING ON THE AVERAGE WEEKS WORKED PER QUARTER BY TRAINING RECIPIENTS, IN DOLLARS
(F Statistics in Parentheses)

	Classroom Training	On-the-Job Training
Quarter		
1	-1.00 ***	2.57 ***
	(13.83)	(15.62)
2	-1.90 ***	4.98 ***
	(28.49)	(33.36)
3	-0.60	4.55 ***
	(2.47)	(23.79)
4	0.74 *	4.69 ***
	(3.64)	(25.07)
Year		
1	-2.76 **	16.79 ***
	(5.79)	(36.39)
2	3.58 **	16.50 ***
	(6.25)	(22.60)
3	2.95 **	18.49 ***
	(4.25)	(28.30)
4	2.26	15.91 ***
	(2.40)	(20.30)
5	2.11	13.63 ***
	(2.06)	(14.55)
6	1.97	12.01 ***
	(1.80)	(11.32)

NOTE: The estimated impacts of training are based on regressions that include training indicators and a set of interaction terms, where the interaction terms are equal to the product of the training indicators and a variety of economic and demographic variables. The hypothesis test used to evaluate the statistical significance of the estimated impact is an F test of the linear equation implied by having the training indicators take a value of 1. The test is calculated according to the assumption that the economic and demographic variables are equal to the means for the training groups. The critical values for the F statistic are 2.71, 3.84, and 6.63 for the 90, 95, and 99 percent confidence levels, respectively.

* Significantly different from zero at the 90 percent confidence level in an F test.
** Significantly different from zero at the 95 percent confidence level in an F test.
*** Significantly different from zero at the 99 percent confidence level in an F test.

The regression-based estimated impacts of on-the-job training on earnings and weeks worked were consistent with these differences. Our findings indicated that on-the-job training had a substantial and statistically significant impact on earnings and weeks worked throughout the six years of followup (see Table III.2 and III.3). The estimated impact on earnings was equal to $9,000 to $15,000 per year; the impacts on weeks worked were 12 to 18 additional weeks per year.

B. UI RECEIPT FOR TRAINEES

The findings on training recipients' UI receipt were generally consistent with the findings on the earnings and employment for this group. Table III.4 shows that classroom trainees received about $4,500 in benefits in the year of their initial claim, compared with about $3,900 for assessed JSA-only claimants. Similarly, weeks of UI benefits collected in the year of the initial claim differed between the two groups: classroom trainees received about 24 weeks, on average, compared with about 21 weeks, on average, for the assessed JSA-only claimants. Thus, classroom trainees received greater benefits during the time they participated in training programs.

After the year of the initial claim, classroom trainees received less UI benefits, on average, than the assessed JSA-only claimants, in terms of both dollars and weeks paid. On average, the overall amount of UI receipt for classroom trainees after the year of the initial claim was about 10 percent lower than the amount received by the assessed JSA-only group during this period (see Table III.4). However, the differences in UI benefits received by the two groups were statistically significant only in the third year. During this year, the classroom trainees received slightly more than two-thirds of the benefits received by the assessed JSA-only claimants. The differences in weeks paid for the two groups were statistically significant in both the third and fourth years when, relative to the JSA-only group, classroom trainees received .7 and .9 fewer weeks of benefits, respectively. The regression estimates of the classroom training impacts on UI benefits and UI weeks paid (See Tables III.5 and Table III.6) are very similar in magnitude and statistical significance to these raw differences in UI receipt.

TABLE III.4

AVERAGE UI RECEIPT FOR TRAINING RECIPIENTS
(Standard Deviations in Parentheses)

	JSA Plus Training or Relocation: Classroom Trainees	JSA Plus Training or Relocation: On-the-Job Trainees	JSA-Only: Assessed Claimants
	UI Benefits		
Year of Initial Claim	4,512 ***	2,589 ***	3,896
(First Year)	(1,284)	(1,176)	(1,533)
Second Year	363	229	370
	(1,179)	(789)	(1,125)
Third Year	273 **	425	405
	(994)	(1,533)	(1,260)
Fourth Year	323	392	450
	(1,278)	(1,627)	(1,379)
Fifth Year	407	612	514
	(1,473)	(1,508)	(1,600)
Sixth Year	438	246	366
	(1,579)	(1,045)	(1,330)
Total Benefits	7,021	5,207 *	6,715
(First to Sixth Year)	(5,117)	(4,979)	(5,560)
	Weeks Paid		
Year of Initial Claim	24.1 ***	15.5 ***	21.3
(First Year)	(4.0)	(7.0)	(6.3)
Second Year	1.7	1.2	2.0
	(5.4)	(3.9)	(5.9)
Third Year	1.5 **	2.0	2.2
	(5.4)	(6.7)	(6.6)
Fourth Year	1.4 **	1.6	2.3
	(5.4)	(6.1)	(6.7)
Fifth Year	1.8	3.4	2.3
	(6.4)	(8.0)	(6.8)
Sixth Year	1.9	1.3	1.7
	(6.5)	(5.3)	(5.7)
Total Weeks	32.5	24.9 **	31.9
(First to Sixth Year)	(15.2)	(19.1)	(19.8)
Number of Observations	**314**	**45**	**1,363**

* Significantly different from the JSA-only treatment mean at the 90 percent confidence level in a two-tail test.

** Significantly different from the JSA-only treatment mean at the 95 percent confidence level in a two-tail test.

*** Significantly different from the JSA-only treatment mean at the 99 percent confidence level in a two-tail test.

TABLE III.5

ESTIMATED IMPACTS OF TRAINING ON THE AVERAGE UI RECEIPT FOR TRAINING RECIPIENTS, IN DOLLARS
(F Statistics in Parentheses)

	Classroom Training	On-the-Job Training
Year of Initial Claim	639 ***	-1,312 ***
	(52.0)	(37.2)
Second Year	-2	-141
	(.0)	(.7)
Third Year	-132 **	12
	(3.0)	(.0)
Fourth Year	-124	-57
	(2.1)	(.1)
Fifth Year	-104	102
	(1.1)	(.2)
Sixth Year	73	-121
	(.7)	(.3)

NOTE: The estimated impacts of training are based on regressions that include training indicators and a set of interaction terms, where the interaction terms are equal to the product of the training indicators and a variety of economic and demographic variables. The hypothesis test used to evaluate the statistical significance of the estimated impact is an F test of the linear equation implied by having the training indicators take a value of 1. The test is calculated according to the assumption that the economic and demographic variables are equal to the means for the training groups. The critical values for the F statistic are 2.71, 3.84, and 6.63 for the 90, 95, and 99 percent confidence levels, respectively.

* Significantly different from zero at the 90 percent confidence level in an F test.
** Significantly different from zero at the 95 percent confidence level in an F test.
*** Significantly different from zero at the 99 percent confidence level in an F test.

TABLE III.6

ESTIMATED IMPACTS OF TRAINING ON THE AVERAGE WEEKS OF UI PAID FOR TRAINING RECIPIENTS
(F Statistics in Parentheses)

	Classroom Training	On-the-Job Training
Year of Initial Claim	2.80 ***	-5.71 ***
	(57.1)	(40.3)
Second Year	-.31	-.83
	(.7)	(.9)
Third Year	-.72 *	-.24
	(3.2)	(.1)
Fourth Year	-.88 **	-.71
	(4.7)	(.5)
Fifth Year	-.49	1.16
	(1.3)	(1.3)
Sixth Year	.20	-.42
	(.3)	(.2)

NOTE: The estimated impacts of training are based on regressions that include training indicators and a set of interaction terms, where the interaction terms are equal to the product of the training indicators and a variety of economic and demographic variables. The hypothesis test used to evaluate the statistical significance of the estimated impact is an F test of the linear equation implied by having the training indicators take a value of 1. The test is calculated according to the assumption that the economic and demographic variables are equal to the means for the training groups. The critical values for the F statistic are 2.71, 3.84, and 6.63 for the 90, 95, and 99 percent confidence levels, respectively.

* Significantly different from zero at the 90 percent confidence level in an F test.
** Significantly different from zero at the 95 percent confidence level in an F test.
*** Significantly different from zero at the 99 percent confidence level in an F test.

Claimants in the JSA plus training or relocation treatment who received on-the-job training received significantly less UI benefits during the year of the initial claim, but the differences in subsequent years were not significant (see Table III.4). The difference in weeks of UI paid for the two groups was equal to six weeks in the year of the initial claim. Annual differences in subsequent years were all small and not statistically significant. These findings are consistent with the findings from the regression estimates of on-the-job training impacts (see Table III.5 and Table III.6).

IV. TARGETING STRATEGIES

A key objective of the New Jersey Unemployment Insurance Reemployment Demonstration Project (NJUIRDP) was to provide reemployment services to workers who were likely to face prolonged spells of unemployment. Program planners reasoned that this group had the greatest need for reemployment services and was most likely to benefit from these services. To achieve this targeting objective, the demonstration used several criteria to screen out those who were likely to obtain a new job quickly, without any assistance.[1]

The evaluation report documented that the demonstration succeeded in targeting a group that, in the absence of services, would have experienced greater-than-average reemployment problems during the first year of followup than did those screened out of the demonstration (Corson et al. 1989). However, it is not clear whether the group NJUIRDP targeted would have experienced greater *long-term* employment problems than noneligibles. This chapter addresses this question, taking advantage of the six years of follow-up data that are now available.

Based in part, on the design and the initial findings from the NJUIRDP, the Unemployment Compensation Amendments of 1993 mandated that states identify workers likely to exhaust UI and refer them to reemployment services. (New Jersey is one of five prototype states that plan to implement this type of targeting system during fall 1994.) The U.S. Department of Labor (USDOL) has suggested that this targeting process, known as worker "profiling," can occur in one of two ways. One option, used in NJUIRDP, is for states to identify specific characteristics for use as screens.

Alternatively, states can use a three-step targeting process, illustrated in a paper produced by USDOL (see paper by Kelleen Worden in USDOL, 1994). The first step involves screening out

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[1]As described in Chapter I, the demonstration excluded those who (1) did not receive a first payment within five weeks after their initial claim or received a partial first payment because they had earnings, (2) were younger than 25, (3) had less than three years of tenure on their pre-UI job, (4) had a definite recall date, or (5) sought work through a union hiring hall.

workers who are not permanently separated--those who have a definite recall date or who seek work through an approved union hiring hall. In the second step, states can use a number of different characteristics to estimate an individual's probability of exhausting UI (this procedure is described in more depth later). In the third step, states refer to reemployment services as many workers as they can, given resource constraints. Workers with higher predicted probabilities of exhausting UI are referred before workers with lower probabilities of exhausting UI. As a result, workers with the highest predicted probabilities of exhausting UI are referred to reemployment services.

In this chapter, we assess the differences between two alternative ways of targeting services to UI claimants: (1) the targeting method used by NJUIRDP; and (2) the new three-step profiling method developed by USDOL. First, we compare the extent to which each of these methods succeeds in targeting workers with employment problems that are more serious than the average for UI claimants. Second, we assess whether the average impacts on employment and UI receipt would have been larger had the demonstration targeted workers in the same manner as the three-step profiling system. In particular, we address the question of whether workers with the largest predicted probabilities of UI exhaustion experienced the largest impacts from NJUIRDP.

To make these assessments, we assume that about 30 percent of the workers passing the initial screens are referred to reemployment services. Since states are expected to refer to reemployment services workers with the highest predicted probabilities of exhaustion, we implement this assumption by assuming that workers with predicted probabilities of exhaustion above the 70th percentile are the ones referred to services. These assumptions only approximate what would happen in actual practice since the threshold between who is referred to reemployment services and who is not referred will vary by office by week depending on available resources and the nature of the caseload. In addition, DOL's plans, as reflected in the proposed Reemployment Act of 1994, call for increased funding for services for dislocated workers. Thus our assumption that 30 percent of the workers passing the initial screens are referred to reemployment services should be viewed as representing initial

application of the worker profiling and reemployment services system. As additional funds for services become available 50 percent or more of the workers passing the initial screens may be referred to services.

The chapter is organized as follows. First, we describe our profiling sample, methodology, and estimates. Second, we compare the short- and long-term employment experiences of several groups, including all UI claimants, NJUIRDP controls, NJUIRDP noneligibles, and workers targeted by the simulated three-step profiling system. Third, we assess whether the NJUIRDP impacts were larger for workers who would be targeted by the three-step profiling system than for other workers.

A. PROFILING SIMULATION

Employing the NJUIRDP data, we followed the three-step profiling process proposed by USDOL and produced results that are similar to those generated by USDOL (USDOL 1994). In general, the relationships between the independent and dependent variables are similar to those found by USDOL. However, our state-specific model has substantially less predictive power than the one estimated by USDOL with national data.

The first subsection describes the sample used to estimate the profiling model. The second subsection discusses the specification of our model and the minor differences between our variables and those used by USDOL. The third subsection reports the results from the model.

1. Samples Used to Estimate the Model

We estimated profiling models using two alternative samples: (1) a relatively large sample (N=3,153), with all of the basic variables needed to estimate our model (the "records sample"); and (2) a substantially smaller subsample (N=1,541), for which additional variables were available (the "survey sample").[2] In each case, we removed from the samples workers who would be screened out

[2]These additional variables were collected through a survey conducted by Mathematica Policy Research, Inc., in 1988 for the demonstration evaluation. As described in the next subsection, the survey sample included additional information on claimants' job tenure and occupation.

by the first of the three profiling steps proposed by USDOL and workers for whom we did not have sufficient data to make this determination. Both samples included NJUIRDP eligibles and a portion of the NJUIRDP noneligibles.

To represent NJUIRDP eligibles in both samples, we used the demonstration control group members. These individuals did not receive any NJUIRDP treatments, so their rate of UI exhaustion was not affected by the demonstration. Since all NJUIRDP eligibles--including all control group members--were permanently separated workers, none of these workers was screened out in the first of the three profiling steps. Within the full population of UI recipients, NJUIRDP eligibles represented 26.6 percent of all claimants (see Figure IV.1).

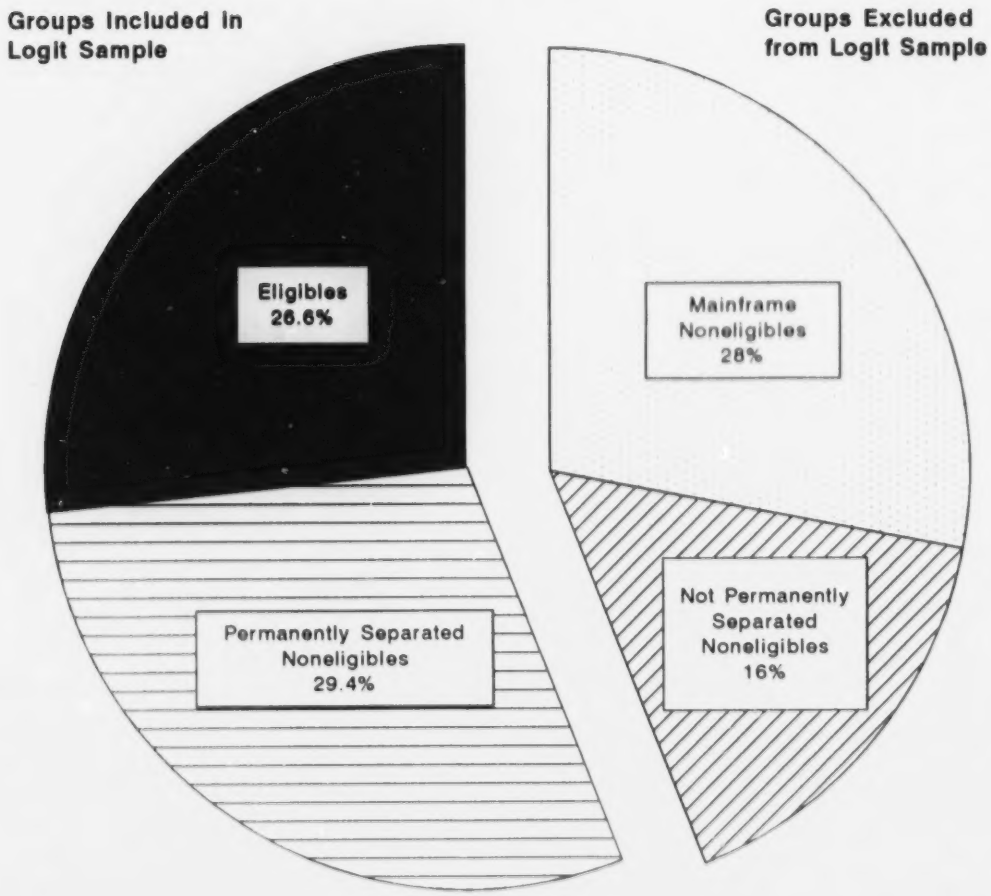
There were two groups of NJUIRDP noneligibles, representing two sets of screens applied sequentially by the demonstration. The first group of workers screened out of the demonstration were the "mainframe noneligibles," workers excluded from NJUIRDP on the basis of data stored on the state's central mainframe computer. The second group of noneligibles, the "Participant Tracking System (PTS) noneligibles", were screened out of NJUIRDP on the basis of data collected in local UI offices and transmitted to the stand-alone computer used to operate the demonstration's tracking system.

To apply the first step in the profiling process, we excluded from the sample all mainframe noneligibles and a portion of PTS noneligibles.[3] The mainframe noneligibles were screened out of the NJUIRDP eligible population for one or more of the following reasons:

- They received partial payments because they had positive earnings.
- They had a gap between the date of their claim filing and their first payment of more than five weeks.
- They were younger than age 25.

[3]Within the full population of UI recipients, the mainframe noneligibles represented 28 percent of all claimants (see Figure IV.1).

FIGURE IV.1
DISTRIBUTION OF UI POPULATION


Groups Included in Logit Sample
Groups Excluded from Logit Sample
Eligibles 26.6%
Mainframe Noneligibles 28%
Not Permanently Separated Noneligibles 16%
Permanently Separated Noneligibles 29.4%

States implementing the new worker profiling policy are expected to exclude workers who received partial payments and had positive earnings because these workers are not permanently separated. States may also exclude workers with a five-week or longer gap between their claim date and the date of the first payment, because delays in the first UI payment make it impossible to meet the objective of early intervention. States will not, however, exclude workers under 25 because states are not allowed to use age as a screen for profiling. Nevertheless, we had to exclude this group from the analysis because we did not have the data on tenure, recall expectations, or union hiring hall status for this group.[4]

We also excluded from the sample PTS noneligibles who we knew were not permanently separated.[5] The remaining NJUIRDP noneligibles--the permanently separated PTS noneligibles--were included in the estimation of the model. All these workers were screened out of the demonstration because they had not worked with their pre-UI employer for three or more years. Within the full population of UI recipients, these nontenured but permanently separated workers represented approximately 30 percent of all claimants (see Figure IV.1).

The actual proportions of records sample members in the various eligible and noneligible groups differ from the proportions shown for the UI population in Figure IV.1 because the noneligible sample selected for the study was approximately the same size as the control group (the eligibles) rather than three times as large as in the UI population. Similarly the subset of noneligibles included in the survey was an even smaller proportion of the survey sample.[6] This underrepresentation of noneligibles may affect our estimates of the effects of claimant characteristics on UI benefit

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[4]In the New Jersey demonstration workers screened out solely on the basis of their age constituted about 10 percent of all UI claimants.

[5]Within, the full population of UI recipients, these workers represented approximately 16 percent of all claimants (see Figure IV.1).

[6]See Appendix A for a full description of these samples.

exhaustion, particularly for job tenure, since sample members with job tenure less that three years come only from the noneligible group.[7]

2. Specification of Our Model

We estimated a profiling model that is similar to the one developed by USDOL for its simulation of the second step in the proposed three-step profiling system (USDOL 1994). We estimated a logit probability model to explain UI exhaustion.[8] We used the same five variables used by USDOL to predict UI exhaustion: (1) education; (2) tenure on the pre-UI job; (3) employment growth in the workers' occupation; (4) employment growth in the workers' industry; and (5) the local unemployment rate. Most of the variables were defined in the same way as those used by USDOL. In particular, we defined the variables as follows:

- ***Education.*** Sample members' education was captured by three categorical dummy variables: (1) no high school diploma; (2) some college; and (3) college degree or more. These variables were set equal to one if they matched the worker's highest level of educational attainment. The coefficients estimated for these three variables represent the difference in exhaustion probabilities as compared to the omitted category--high school diploma.

- ***Job Tenure.*** The survey sample model included the three job tenure categorical variables used by USDOL: (1) pre-UI job tenure of 3 to 5 years; (2) 6 to 9 years; and (3) 10 or more years. In the records sample model, because of data limitations, we used only a single binary variable, pre-UI job tenure of three or more years. The coefficients estimated for these variables represent the difference in exhaustion probabilities as compared to the omitted category--pre-UI job tenure less than three years.

- ***Occupation Growth.*** This variable was set equal to one if employment in the worker's pre-UI occupation was growing during the previous year and equal to zero otherwise. We used occupational employment data for the mid-Atlantic region.[9] Occupations were

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[7]We use weights to account for this underpresentation of noneligibles whenever we derive mean outcomes for groups that include both eligibles and noneligibles. However, we do not use any weights for estimating the UI exhaustion model since weights would not adjust for potential bias in the estimates.

[8]Because its data set did not contain information on UI exhaustion, USDOL used a categorical dependent variable equal to one if a worker's unemployment spell was six months or longer.

[9]Because of data limitations, the USDOL model used occupational data aggregated to the national level.

grouped into six categories: (1) managerial and professional; (2) technicians, sales, and administration; (3) services; (4) precision production, craft, and repair; (5) operators, fabricators, and laborers; and (6) farming, forestry, and fishing. Information on claimants' occupation was available only for the survey sample; hence, only the survey sample model included the occupational growth variable.

- ***Employment Growth in the Worker's Pre-UI Industry.*** Industries were aggregated into nine different groupings: (1) mining and construction; (2) durable manufacturing; (3) nondurable manufacturing; (4) transportation and utilities; (5) wholesale; (6) retail; (7) finance, insurance, and real estate; (8) services; and (9) government. We used industry growth rates pertaining to substate labor market areas for which detailed industry employment data are available. The 10 demonstration sites fell into 6 of the state's 11 labor market areas for which data are available.

- ***Unemployment Rate.*** We used local 1986 unemployment rates for the same six substate labor market areas.

3. Estimates of Model and Exhaustion Probabilities

a. Basic Survey and Full Record Sample Models

All but one of the coefficients estimated for the full records sample had the expected signs (see Table IV.1). Like the estimates derived by USDOL, UI exhaustion was positively related to both tenure and the unemployment rate and negatively related to industry employment growth and higher levels of education. The one unanticipated result was the negative sign on the "no high school diploma" coefficient, suggesting that high school dropouts are less likely to exhaust UI than are high school graduates. However, this coefficient was not statistically significant.

Only the "college degree" and "local unemployment rate" coefficients were significantly different from zero. In contrast, all the coefficients estimated by USDOL with a national data set were statistically significant (USDOL 1994). Hence, this model which is estimated from a single state has less predictive power than the USDOL model which was estimated using data for a national sample.

Most of the survey sample's estimated coefficients were consistent with those of the records sample (see Table IV.2). Like the records sample estimates, the survey sample "no high school diploma" coefficient had an unanticipated negative sign. In addition, "occupational employment growth" had a unanticipated positive sign. However, neither of these coefficients was statistically

TABLE IV.1

LOGIT ESTIMATION OF PROBABILITY OF UI EXHAUSTION, RECORDS SAMPLE[a]

Independent Variable	Mean of Independent Variable	Coefficient	Standard Error	Change in Probability per Unit Change of Independent Variable (Percentage Points)[c]
No High School Diploma	.334	-.110	.086	-2.7
High School Diploma	.412	[b]	[b]	[b]
Some College	.140	-.060	.112	-1.5
College Degree	.114	-.298 **	.123	-7.2
Tenure Less than 3 Years	.262	[b]	[b]	[b]
Tenure 3 or More Years	.738	.070	.083	1.7
Industrial Employment Change	.832	-.008	.007	-.2
Local Unemployment Rate	5.474	.124 ***	.027	3.0
Constant	--	-.870 ***	.171	--

NOTE: Sample includes 2,252 control group members and 901 noneligibles.

The unweighted mean value of the dependent variable (the probability of UI exhaustion) is .445.

The -2 log likelihood is 33.34 with a p value of .0001.

[a]Dependent variable is assigned value of 1 if exhausts UI; 0 otherwise.

[b]Omitted category for dummy variables.

[c]Evaluated at mean of independent variable.

* Coefficient significantly different from zero at the 90 percent confidence level, two-tailed test.
** Coefficient significantly different from zero at the 95 percent confidence level, two-tailed test.
*** Coefficient significantly different from zero at the 99 percent confidence level, two-tailed test.

TABLE IV.2

LOGIT ESTIMATION OF PROBABILITY OF UI EXHAUSTION, SURVEY SAMPLE[a]

Independent Variable	Mean of Independent Variable	Coefficient	Standard Error	Change in Probability per Unit Change of Independent Variable (Percentage Points)[c]
No High School Diploma	.291	-.086	.128	-2.08
High School Diploma	.432	[b]	[b]	[b]
Some College	.149	-.165	.158	-3.97
College Degree	.128	-.226	.169	-5.42
Tenure Less than 3 Years	.225	[b]	[b]	[b]
Tenure 3 to 5 Years	.245	.111	.154	2.603
Tenure 6 to 9 Years	.219	.339 **	.157	8.123
Tenure 10 or More Years	.311	.523 ***	.147	12.69
Industrial Employment Change	.549	-.012	.010	-.27
Occupational Employment Growth	.790	.130	.134	3.12
Local Unemployment Rate	5.300	.096 **	.040	2.31
Constant	--	-1.058 ***	.274	--

NOTE: Sample includes 1,372 control group members and 169 noneligibles.

The unweighted mean value of the dependent variable (the probability of UI exhaustion) is .435.

The -2 log likelihood is 30.20 with a p value of .0004.

[a]Dependent variable is assigned value of 1 if exhausts UI; 0 otherwise.

[b]Omitted category for dummy variables.

[c]Evaluated at mean of independent variable.

* Coefficient significantly different from zero at the 90 percent confidence level, two-tailed test.
** Coefficient significantly different from zero at the 95 percent confidence level, two-tailed test.
*** Coefficient significantly different from zero at the 99 percent confidence level, two-tailed test.

significant. The variables that were statistically significant (at the 95 percent level of confidence) were "tenure of 6 to 9 years", "tenure of 10 or more years", and "local unemployment rate."

Although relatively few of either models' coefficients are statistically significant, these models are still useful in directing reemployment services to workers with relatively high probabilities of UI benefit exhaustion (see Table IV.3). As shown in the table, the initial screens used in the profiling model by themselves divide workers into two groups that differ substantially in their likelihood of exhaustion. In the records sample, workers screened out in the first step had a mean probability of exhaustion of 30 percent as compared to the remaining workers who had a mean probability of exhaustion of 44 percent. Use of the probability model further identifies a group of workers with a relatively high likelihood of exhaustion. For example, if the probability model were used to refer about 30 percent of the workers passing the initial screens to reemployment services, these workers would in general be those with predicted probabilities of exhaustion above the 70th percentile because states are expected to refer to reemployment services workers with the highest predicted probabilities of exhaustion. In this case, the actual mean probability of exhaustion among workers referred to reemployment services would be about 53 percent. The workers not referred to reemployment services would have a probability of exhaustion of 41 percent, a difference of 12 percentage points.[10]

A final point to note about the probability models is that a substantial proportion of the workers with the highest predicted probabilities of exhaustion are workers who were screened out of the New Jersey demonstration because of the tenure screen. Specifically, 42 percent of the records sample members with predicted probabilities exceeding the 70th percentile were NJUIRDP noneligibles.

[10]The results for the model estimated for the survey sample are similar (see Table IV.3).

TABLE IV.3

MEAN EXHAUSTION PROBABILITIES FOR GROUPS TARGETED AND NOT TARGETED BY WORKER PROFILING

	Record Sample Model	Survey Sample Model
Mainframe Noneligibles and Not Permanently Separated Workers	29.7	28.6
Permanently Separated Workers	44.1	40.3
Below 70th percentile	40.5	35.2
Above 70th percentile	52.5	52.2

b. Elaboration of the Model

The worker profiling guidelines (see Unemployment Insurance Program Letter No. 41-94 in USDOL 1994) issued to states by the federal government require states to use initial screens related to receipt of a first payment, recall status, and union hiring hall status and to use information on industry or occupation in the second step. States are also permitted to use additional variables in the second step (including the tenure, education, and local unemployment rate variables used in Worden 1994) as long as the variables used in this step are not discriminatory. Specifically, states cannot use age, race, ethnic group, sex, color, national origin, disability, religion, political affiliation, or citizenship.

Given this situation, we present, in this section, an additional exhaustion model that includes all the variables used in the basic model and several additional explanatory variables that might be used. Whether these other variables should be added to a profiling model depends largely on whether these variables are good predictors of UI exhaustion. These additional explanatory variables are: (1) base-year earnings; (2) the UI weekly benefit amount; and (3) a categorical variable equal to one if a worker expects to be recalled but does not have a definite recall date (see Table IV.4).[11]

When we estimated the model on the record sample, we found that UI exhaustion was negatively related to base-year earnings and workers' expectation that they would be recalled; these negative relationships were statistically significant at the 95 percent confidence level. Exhaustion was also positively related to the UI weekly benefit amount; this positive relationship was statistically significant at the 90 percent confidence level. Including these three additional variables also increased the magnitude of the coefficient for "tenure of three or more years"; so that this coefficient became statistically significant at the 90 percent confidence level.

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[11]These variables are defined as follows: (1) earnings in the base year include all earnings in the first four of the five calendar quarters preceding the claim date; (2) the weekly benefit amount paid to UI recipients is expressed in hundreds of dollars; and (3) workers who expected that they would be recalled, but who had no definite recall date are included in this category (workers with a definite recall date were excluded in step one).

TABLE IV.4

LOGIT ESTIMATION OF PROBABILITY OF UI EXHAUSTION
(RECORDS SAMPLE),
ELABORATED MODEL[a]

Independent Variable	Mean of Independent Variable	Coefficient	Standard Error	Change in Probability per Unit Change of Independent Variable (Percentage Points)[c]
No High School Diploma	.334	-.033	.089	-.8
High School Diploma	.412	[b]	[b]	[b]
Some College	.140	-.047	.113	-1.1
College Degree	.114	-.186	.130	-4.5
Tenure Less than 3 Years	.262	[b]	[b]	[b]
Tenure 3 or More Years	.738	.161 *	.086	3.8
Industrial Employment Change	.832	-.009	.007	-.2
Local Unemployment Rate	5.474	.131 ***	.028	3.2
Base-Year Earnings ($1,000s)	16.643	-.022 ***	.005	-.5
Weekly Benefit Amount ($100s)	1.762	.191 *	.102	4.6
Expect Recall	.341	-.533 ***	.084	-12.7
Constant	--	-.804 ***	.227	--

NOTE: Sample includes 2,252 control group members and 901 noneligibles.

The unweighted mean value of the dependent variable (the probability of UI exhaustion) is .445.

The -2 log likelihood is 88.46 with a p value of .0001.

[a]Dependent variable is assigned value of 1 if exhausts UI; 0 otherwise.

[b]Omitted category for dummy variables.

[c]Evaluated at mean of independent variable.

* Coefficient significantly different from zero at the 90 percent confidence level, two-tailed test.
** Coefficient significantly different from zero at the 95 percent confidence level, two-tailed test.
*** Coefficient significantly different from zero at the 99 percent confidence level, two-tailed test.

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Including the three variables enhanced the model's ability to identify those who were likely to exhaust UI, using the same benchmark described earlier. Those above the 70th percentile of the probability distribution had a 55 percent exhaustion rate, which is 16 percentage points above the exhaustion rate of those below the threshold (39 percent). This difference in exhaustion rates was four percentage points greater than the difference generated by the model that did not include the three additional variables.

The added predictive power of the three additional variables suggests that states might consider adding one or more of them to USDOL's basic profiling model. However, it may be impractical to implement a profiling system that uses workers' recall expectations as a screening mechanism. If it becomes widely known that this variable is being used to target reemployment services, workers may change their response to questions about their recall status.[12] In addition, those who expected to be recalled experienced larger impacts of the UIRDP treatments during the first year of followup (Anderson et al. 1991). If workers' recall expectations are included in the profiling model, those who expect to be recalled will be less likely to receive reemployment services, reducing average impacts, at least in the short term.[13]

B. LONG-RUN IMPLICATIONS OF ALTERNATIVE TARGETING STRATEGIES

The NJUIRDP attempted to provide services to workers who were likely to experience difficulty obtaining a job without some assistance. On the basis of the first year of followup, it appeared that the NJUIRDP succeeded in targeting workers who, in the absence of any intervention, would have remained unemployed for a longer period of time than workers who were not eligible to participate

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[12]One way of addressing this issue would be to refer workers to reemployment services who indicated that they expected recall but who remained unemployed after some period, say 13 weeks.

[13]Second-year impacts were also larger for those who expected to be recalled than they were for those who did not expect to be recalled; however, this difference was not statistically significant. As noted in Chapter II, controls who expected to be recalled were much more likely to receive UI in the second year than controls who did not expect to be recalled, suggesting that this group was largely responsible for the second-year impacts.

(Corson et al. 1989). Control group members--who represent the eligible group--received an average of 17.9 weeks of UI benefits during the year of their initial claim, compared with 15.1 weeks for noneligibles. Similarly, the control group was employed 22.8 weeks during the first year of followup, compared with 26.4 weeks for noneligibles.

Presumably, policymakers are concerned about both the short-run and the long-run experiences of workers targeted for specific services. Now that six years of follow-up data are available, it is possible to determine whether, in the absence of any intervention, the population targeted by NJUIRDP would have experienced greater long-run employment problems than those screened out of the demonstration. This issue is addressed in the next subsection. In addition to employment and UI receipt, we compare NJUIRDP eligibles' pre- to post-UI earnings reductions with those experienced by unemployed workers screened out of the demonstration.

The profiling model developed by USDOL targets services to UI recipients who are likely to exhaust UI. However, it is not known whether those likely to exhaust UI are also likely to experience relatively severe long-term reemployment problems. The second subsection addresses this issue by comparing the experiences of UI recipients targeted by our simulated profiling system with the experiences of other UI recipients. For this analysis, we relied entirely on the estimates from the basic profiling model (estimated with records data), rather than those derived from the elaborated model with the three additional exhaustion predictors.

1. Long-Run UI Receipt and Earnings for Those Targeted by NJUIRDP

The demonstration's targeting strategy succeeded in serving workers who would have experienced, in the absence of any NJUIRDP services, substantial earnings reductions. Moreover as shown in Table IV.5, the reductions in earnings experienced by eligibles (the control group) were substantially greater than the average earnings reductions for noneligibles, the group that was excluded by NJUIRDP's targeting system. The difference between the base-year and current-year earnings of eligibles was approximately $9,000 in the first year of followup and exceeded $6,000 in

TABLE IV.5

LONG-RUN UI RECEIPT AND EARNINGS

UI and Earnings Outcomes	All UI Recipients[a]	NJUIRDP Targeting		Profiling Targeting			
		Targeted Group (Controls)	Noneligibles	Targeted Group: Above 70th Percentile of Exhaustion Probabilities[a]	Noneligibles: Below 70th Percentile of Exhaustion Probabilities[a]	Noneligibles: Not Permanently Separated[b]	Noneligibles: Mainframe Noneligibles
Year 1 Outcomes							
Weeks Employed	25.4	22.8	26.4 ###	22.7	23.9	31.9 ***	25.8 ***
Weeks on UI	15.9	17.9	15.1 ###	18.6	16.7 ***	13.1 ***	14.5 ***
Percentage Exhausting UI	37.9	44.7	35.4 ###	52.5	40.5 ***	21.1 ***	34.6 ***
Difference Between Annual Earnings and Base-Period Earnings							
Year 1	-4,776	-9,073	-3,217 ###	-5,838	-7,024 ***	1,719 ***	-2,678 ***
Year 2	-3,128	-6,656	-1,848 ###	-4,026	-4,721	-1,292 ***	-1,181 ***
Year 3	-2,629	-6,078	-1,377 ###	-3,752	-4,235	-1,425 ***	-536 ***
Year 4	-2,672	-6,230	-1,381 ###	-3,626	-4,356	-1,774 ***	-425 ***
Year 5	-3,028	-6,262	-1,855 ###	-4,129	-4,485	-3,184	-458 ***
Year 6	-3,319	-6,721	-2,112 ###	-4,404	-4,550	-3,756	-912 ***
Long-Term Outcomes (Year 1 through Year 6)							
Number of Times on UI[c]	2.07	1.84	2.15 ###	1.95	1.76 ***	2.89 ***	2.08 **
Number of Weeks Regular UI	32.7	31.9	33.0	35.4	29.4 ***	41.5 ***	30.6 ***
Number of Weeks Regular and Extended UI	37.2	35.7	37.7 ##	39.8	33.1 ***	46.8 ***	35.5 ***
Number of Weeks Worked Per Year	25.0	24.4	25.3	23.3	24.5 *	27.6 ***	25.3 ***
Return to Largest Base Period Employer for 2 or More Quarters	25.5	23.8	26.0	24.2	16.4 ***	50.3 ***	23.6
Percentage of UI Population	100	26.6	73.4	16.8	39.2	16	28

[a]The controls and noneligibles included in these groups are weighted to reflect the relative shares of eligibles and noneligibles in the UI population.

[b]The "not permanently separated" noneligibles are those who had a definite recall date or were members of a union with a hiring hall.

[c]This variable is a count of the number of UI benefit years established following the NJUIRDP claim.

Significantly different from comparable figure for NJUIRDP targeted group at the 90 percent level of confidence (two-tailed test).
Significantly different from comparable figure for NJUIRDP targeted group at the 95 percent level of confidence (two-tailed test).
Significantly different from comparable figure for NJUIRDP targeted group at the 99 percent level of confidence (two-tailed test).

* Significantly different from comparable figure for the profiling targeted group at the 90 percent level of confidence (two-tailed test).
** Significantly different from comparable figure for the profiling targeted group at the 95 percent level of confidence (two-tailed test).
*** Significantly different from comparable figure for the profiling targeted group at the 99 percent level of confidence (two-tailed test).

each subsequent year. In contrast, the average earnings reduction for noneligibles was $3,200 in the first year and less than $2,200 in each subsequent year. The differences between the earnings reductions of eligibles and noneligibles were statistically significant in each year of followup. The reason for these differences are probably attributable to eligibles' higher incomes during the base-year period; hence, eligibles' incomes could drop by a larger margin.[14] The average number of weeks of employment per year and the likelihood of returning to the base period employer were also lower for eligibles than noneligibles,[15] but the differences between the two groups were small and not statistically significant.

Although NJUIRDP targeted a group that experienced relatively large earnings reductions, this group also received UI for a shorter amount of time than noneligibles. During the six years following their random assignment, eligibles received UI benefits (both regular and extended) for a total of 35.7 weeks, compared with 37.7 for noneligibles.[16] UI receipt was particularly high (46.8 weeks) among noneligibles who were not permanently separated from their jobs, either because they had a definite recall date or because they tended to secure employment through a hiring hall. The group of noneligibles who were not permanently separated tended to be on UI frequently; this group included individuals, such as construction workers, in industries characterized by seasonal or unstable employment.

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[14]The average base-year earnings of eligibles and noneligibles were $18,046 and $13,144, respectively. It is likely that eligibles' relatively large amount of job tenure contributed to their large earnings reductions.

[15]We defined "return to largest base period employer" to be the presence of two or more quarters of earnings from the largest base period employer reported during the six years after the initial UI claim. We used this definition because there were a large number of eligibles with one quarter of earnings from the base period employer. These earnings may have been severance pay.

[16]This difference was statistically significant at the 95 percent level of confidence.

2. Long-Run UI Receipt and Earnings for Those Targeted by Worker Profiling and Reemployment Services Systems

Worker profiling and reemployment services systems will target services to permanently separated workers with the highest probabilities of exhaustion. If we assume, for analysis purposes, that this targeted group includes about 30 percent of the workers passing the initial screens, we find that during the six years of followup, this targeted group had an average of approximately three additional weeks of UI receipt and three fewer weeks of employment than the average for all UI recipients (see Table IV.5). In each year of the six years of followup, the average earnings reductions for the targeted group were at least $800 more than the average reduction for all UI recipients.

When we compare the group targeted by worker profiling to the group targeted by the NJUIRDP, we find that the profiling group had not only a higher probability of exhausting UI in the initial year, as expected, but a greater likelihood of receiving UI in subsequent years (the two groups received UI for approximately 40 and 36 weeks, respectively, during the six years of followup). Similarly weeks worked per year were lower for the profiled group than for NJUIRDP eligibles (the control group). On the other hand, earnings reductions for the targeted group were smaller than those sustained by the NJUIRDP eligibles, a finding that is probably due to the fact that the targeted group had fewer years of job tenure than the NJUIRDP eligibles.

If we compare the group targeted by worker profiling to the groups who would not be referred to services under worker profiling (those below the 70th percentile of exhaustion probabilities, those not permanently separated, and mainframe noneligibles), we find that the targeted group had, with two exceptions, fewer weeks of work, greater earnings losses, and more unemployment insurance receipt throughout our six year observation period. The first exception to this pattern is that year one earnings reductions were higher and the likelihood of returning to the largest base period employer were lower for the group below the 70th percentile than they were for the targeted group. However, earnings reductions of the two groups were similar in subsequent years and the likelihood of UI receipt was higher for the group targeted by profiling. The second exception to the general

pattern was that the workers who were excluded from profiling because they had a recall date or used a union hiring hall (the not permanently separated group) were significantly more likely to receive UI in subsequent years than the group targeted by profiling. Other measures in the table (on earnings losses and return to the base period employer) suggest, however, that this group was job attached and that their frequent collection of UI was likely due to the nature of their jobs.[17]

C. IMPACTS OF PROFILED AND NONPROFILED WORKERS

A fundamental question for the proposed profiling policy is whether the system will succeed in targeting reemployment services to those who are most likely to benefit from services. A related question is whether the average NJUIRDP impacts could have been increased had the demonstration used the proposed three-step profiling method for targeting services. We addressed these questions by estimating the difference between the NJUIRDP impacts experienced by workers who would be targeted by the simulated profiling system and those who would be excluded under this system.

For the purposes of estimating these subgroup differences in impacts, we assumed, as earlier, that reemployment services would, in general, be offered to individuals with probabilities of exhaustion above the 70th percentile.[18] Those with predicted probabilities above the 70th percentile were designated as "targeted workers," while those with predicted probabilities below this level were designated as "nontargeted workers." We estimated the impact differences by including in the impact regressions three additional interaction terms. These terms represented the product of a categorical profiling variable (indicating whether or not a worker was targeted) with each of the three treatment

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[17]The not permanently separated group had the highest probability (50 percent) of return to the largest base period employer of any group and the smallest earnings reduction in the first year. The mainframe noneligibles had smaller earnings reductions in subsequent years. This situation probably arises because all claimants under age 25 were in the mainframe noneligible group.

[18]As discussed earlier, this example is best viewed as representing the initial application of worker profiling. As additional funds for services become available, it is likely that a larger group would be referred to services.

categorical variables (which were equal to one if the worker was assigned to that treatment group).[19] We also added the categorical profiling variable to control for the profiling status of the workers.

When we estimate earnings and UI impacts for the targeted and nontargeted workers (see Tables IV.6 and IV.7), we find some statistically significant reductions in UI receipt and increases in earnings for both the targeted and nontargeted groups. These significant impacts mirror those reported in Chapter II; the impacts occur primarily in year's one and two and most of the significant impacts are found for UI receipt. Interestingly, the estimated reductions in UI weeks collected and benefits for the targeted group tend to be larger than those for the nontargeted group and the impacts for the target group are also more likely to be statistically significant. Nevertheless, none of the differences in impacts between the targeted and nontargeted groups is statistically significant. This is not surprising, since splitting the sample into targeted and nontargeted groups lowers the effective sample sizes used to generate impact estimates.

These findings, while not conclusive, suggest that using a profiling model to target reemployment services on workers with high probabilities of UI benefit exhaustion directs reemployment services to a group of workers who are likely to benefit from the services.[20] These estimates also imply that this approach to targeting services is a relatively efficient way to provide services. Services are directed to a specific group of displaced workers who can benefit more from the services than a broader group of displaced workers, thereby generating relatively large savings in UI receipt for the given level of expenditures on services.

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[19]The three treatment groups were JSA only, JSA plus training or relocation, and JSA plus re-employment bonus.

[20]These findings should be treated with caution since the sample sizes are relatively small and since we could not include in the analysis all groups who would be offered reemployment services under worker profiling. Specifically, younger workers and workers with job tenure under three years were not offered services in the New Jersey demonstration. Some of these workers would be offered services under worker profiling.

TABLE IV.6

TARGETED AND NONTARGETED WORKERS' IMPACTS
ON EARNINGS AND WEEKS WORKED
(Standard Error in Parentheses)

	JSA Only			JSA Plus Training or Relocation			JSA Plus Re-Employment Bonus		
	Nontargeted	Targeted	Difference	Nontargeted	Targeted	Difference	Nontargeted	Targeted	Difference
Earnings (In Dollars)									
Year 1	135 (375)	116 (151)	251 (628)	100 (341)	-98 (431)	2 (566)	643 * (375)	-989 (1,661)	-346 (624)
Year 2	329 (459)	-282 (462)	47 (769)	-208 (417)	327 (2,035)	119 (695)	212 (459)	-444 (1,404)	-232 (765)
Year 3	418 (501)	-1,133 (2,566)	-715 (839)	-4 (455)	-301 (588)	-305 (757)	353 (500)	-1,104 (1,854)	-751 (835)
Year 4	399 (523)	-946 (4,474)	-547 (876)	5 (475)	25 (442)	30 (791)	672 (522)	-1,311 (27,336)	-639 (871)
Year 5	29 (551)	227 (589)	256 (921)	-221 (501)	592 (2,624)	371 (833)	96 (551)	-313 (1,897)	-217 (918)
Year 6	279 (565)	-305 (914)	-26 (947)	156 (513)	140 (211)	296 (854)	291 (565)	-179 (334)	112 (942)
(Total Years 1 to 6)	**1,589** **(2,574)**	**-2,324** **(9,409)**	**-735** **(4,312)**	**-172** **(2,338)**	**686** **(6,236)**	**514** **(3,889)**	**2,268** **(2,571)**	**-4,340** **(75,549)**	**-2,072** **(4,289)**
Weeks Worked (In Weeks)									
Year 1	0.33 (0.68)	0.94 * (0.54)	1.27 (1.14)	-0.01 (0.62)	0.73 (0.85)	0.72 (1.03)	0.63 (0.68)	0.24 * (0.14)	0.87 (1.14)
Year 2	0.65 (0.79)	-0.61 (0.94)	0.04 (1.32)	-0.47 (0.72)	0.90 (17.65)	0.43 (1.19)	-0.02 (0.79)	0.20 (1.38)	0.18 (1.32)
Year 3	0.68 (0.82)	-1.57 (8.51)	-0.89 (1.37)	-0.35 (0.74)	0.73 (29.80)	0.38 (1.23)	0.15 (0.81)	0.16 (0.38)	0.31 (1.35)

TABLE IV.6 *(continued)*

	JSA Only			JSA Plus Training or Relocation			JSA Plus Re-Employment Bonus		
	Nontargeted	Targeted	Difference	Nontargeted	Targeted	Difference	Nontargeted	Targeted	Difference
Year 4	1.34 (0.83)	-3.01 (10.01)	-1.67 (1.39)	-0.11 (0.75)	0.83 (1.36)	0.72 (1.25)	0.83 (0.83)	-1.38 (5.41)	-0.55 (1.38)
Year 5	1.17 (0.83)	-1.84 (4.19)	-0.67 (1.40)	0.33 (0.75)	0.37 (0.36)	0.70 (1.25)	0.71 (0.83)	-1.36 (24.04)	-0.65 (1.39)
Year 6	0.69 (0.83)	-0.88 (1.97)	-0.19 (1.39)	-0.08 (0.76)	1.38 (1.13)	1.30 (1.26)	0.27 (0.83)	0.42 (0.48)	0.69 (1.39)
(Total Years 1 to 6)	**4.86** **(4.03)**	**-6.99** **(13.86)**	**-2.13** **(6.76)**	**-0.70** **(3.66)**	**4.95** **(6.79)**	**4.25** **(6.09)**	**2.57** **(4.03)**	**-1.71** **(2.68)**	**0.86** **(6.72)**

NOTE: The difference is the targeted estimated minus the nontargeted estimate.

* Coefficient significantly different from zero at the 90 percent confidence level, two-tailed test.
** Coefficient significantly different from zero at the 95 percent confidence level, two tailed test.

\# Targeted and nontargeted coefficients significantly different at the 90 percent confidence level, two-tailed test.
\#\# Targeted and nontargeted coefficients significantly different at the 95 percent confidence level, two-tailed test.

TABLE IV.7

TARGETED AND NONTARGETED WORKERS' IMPACTS
ON UI WEEKS PAID AND UI DOLLARS RECEIVED
(Standard Error in Parentheses)

	JSA Only			JSA Plus Training or Relocation			JSA Plus Re-Employment Bonus		
	Nontargeted	Targeted	Difference	Nontargeted	Targeted	Difference	Nontargeted	Targeted	Difference
UI Weeks Paid (In Weeks)									
Year 1	-0.391	0.583	0.192	-0.309	-0.736 *	-0.427	-0.763 **	-1.297 ***	-0.534
	(0.322)	(0.433)	(0.540)	(0.293)	(0.389)	(0.877)	(0.322)	(0.430)	(0.537)
Year 2	-0.615 **	-0.385	0.230	-0.331	0.721	0.390	-0.381	0.281	-0.100
	(0.254)	(0.342)	(0.426)	(0.231)	(3.779)	(0.385)	(0.254)	(0.198)	(0.424)
Year 3	-0.028	0.101	0.129	-0.092	0.083	-0.009	-0.116	-0.380 *	-0.496
	(0.259)	(0.350)	(0.434)	(0.235)	(0.258)	(0.391)	(0.259)	(0.214)	(0.431)
Year 4	0.145	0.311 *	0.456	-0.076	0.196	0.120	0.184	-0.30	-0.116
	(0.262)	(0.182)	(0.438)	(0.238)	(1.382)	(0.395)	(0.261)	(1.523)	(0.436)
Year 5	0.183	-0.421	-0.238	-0.002	-0.025	-0.027	0.365	-0.730	-0.365
	(0.264)	(0.365)	(0.442)	(0.240)	(0.274)	(0.399)	(0.264)	(10.000)	(0.440)
Year 6	-0.062	0.407	0.345	0.024	0.288	0.312	-0.11	0.035	-0.076
	(0.236)	(0.457)	(0.396)	(0.215)	(0.245)	(0.357)	(0.236)	(0.059)	(0.394)
(Total Years 1 to 6)	**-0.768**	**1.498**	**0.730**	**-0.786**	**1.145**	**0.359**	**-0.823**	**-0.865 ** **	**-1.688**
	(0.848)	**(4.516)**	**(1.420)**	**(0.770)**	**(2.749)**	**(1.281)**	**(0.847)**	**(0.390)**	**(1.430)**
UI Receipt (In Dollars)									
Year 1	-75	-182 **	-108	-47	-165 **	-117	-144 **	-208 **	-65
	(68)	(91)	(114)	(62)	(82)	(103)	(68)	(91)	(113)
Year 2	-105 **	-71	34	-49	93	44	-67	48	-19
	(48)	(64)	(81)	(44)	(1,040)	(73)	(48)	(36)	(8[illegible]
Year 3	-9	-3	-12	-7	-15	-22	-12	-123 **	-135
	(51)	(3)	(86)	(47)	(32)	(78)	(51)	(57)	(86)

TABLE IV.7 *(continued)*

	JSA Only			JSA Plus Training or Relocation			JSA Plus Re-Employment Bonus		
	Nontargeted	Targeted	Difference	Nontargeted	Targeted	Difference	Nontargeted	Targeted	Difference
Year 4	-2	87	85	-22	48	26	33	-82	-49
	(56)	(79)	(94)	(51)	(769)	(85)	(56)	(381)	(93)
Year 5	38	-85	-47	4	-29	-25	85	-163	-78
	(61)	(481)	(102)	(55)	(103)	(92)	(61)	(1,962)	(101)
Year 6	-31	135	104	-3	92	89	-30	20	-10
	(56)	(139)	(94)	(51)	(72)	(85)	(56)	(37)	(94)
(Total Years 1 to 6)	**-184**	**240**	**56**	**-124**	**119**	**-5**	**-136**	**-220 ****	**-356**
	(177)	**(447)**	**(298)**	**(162)**	**(198)**	**(269)**	**(178)**	**(106)**	**(296)**

NOTE: The difference is the targeted estimate minus he nontargeted estimate.

* Coefficient significantly different from zero at the 90 percent confidence level, two-tailed test.
** Coefficient significantly different from zero at the 95 percent confidence level, two tailed test.
*** Coefficient significantly different from zero at the 99 percent confidence level, two-tailed test.

\# Targeted and nontargeted coefficients significantly different at the 90 percent confidence level, two-tailed test.
\#\# Targeted and nontargeted coefficients significantly different at the 95 percent confidence level, two-tailed test.

V. BENEFIT-COST ANALYSIS

In this chapter, we combine estimates of the impacts of the New Jersey Unemployment Insurance Reemployment Demonstration Project (NJUIRDP) presented in earlier chapters with estimates of the demonstration's net costs to assess whether, compared with the existing UI system, the benefits of each treatment exceeded its costs. We also assess benefits and costs from several other perspectives--those of the major groups affected by the demonstration policies (claimants, employers, and the government) and of society as a whole. This process summarizes the information from the evaluation to help policymakers determine the relative desirability of providing any of these treatments on an ongoing basis.

Our benefit-cost evaluation addresses several issues:

- The costs of providing each of the three treatments on an ongoing basis, relative to the costs of existing services (referred to as "net costs")
- The effects of each treatment, compared with those of existing services, from the perspectives of society as a whole, claimants, employers, and the government (referred to as "net effects")--that is, whether benefits outweigh costs or vice versa
- Whether the offer of training and relocation assistance or the offer of the reemployment bonus generated benefits that exceeded the costs of these additional services
- How the benefits and costs of the treatments are allocated among U.S. Department of Labor (USDOL) programs--that is, Unemployment Insurance (UI), the Employment Service (ES), and Job Training Partnership Act (JTPA) programs at the local, state, and federal levels--and the rest of the government sector

In the first follow-up evaluation, we presented the results of a benefit-cost analysis of the NJUIRDP based on the impacts measured during the year of the initial UI claim and the following two years. The results of this earlier analysis showed that all three treatments offered net benefits to society as a whole and to claimants, when compared with existing services. The JSA-only and JSA plus reemployment bonus treatments also led to net gains for the government sector as a whole and

to the Labor Department agencies that actually offered the services. The JSA plus training or relocation treatment was expensive for the government sector.

Here, we extend the benefit-cost analysis by incorporating the longer-term impacts described in previous chapters. Because the longer-term impacts provided evidence that the treatments generated additional UI savings, the results of the benefit-cost analysis are more favorable than they were in the first followup.[1] We used several approaches to incorporate the long-term impacts into the benefit-cost analysis. Our basic approach used only the long-term impacts on regular UI receipt to extend the analysis, because we are confident that the impacts on regular UI receipt represent real impacts of the demonstration. In this approach, as in Corson et al. (1989), we used the estimated impacts on earnings based on the follow-up interview in the analysis, thereby assuming implicitly that all impacts on earnings took place before the follow-up interview. We then modified this approach in two ways. First, we incorporated impacts for all Unemployment Compensation programs--regular UI, Emergency Unemployment Compensation (EUC), and New Jersey's two extended benefits programs--even though we view the impacts of these programs with some uncertainty (see discussion in Chapter II). Second, we used the estimates of the long-term impacts on earnings that were presented in Chapter II. These earnings impacts were based on data from quarterly wage records. Because we have wage records for six years following the period of the demonstration, we were able to examine the possibility that the treatments affected the earnings of claimants over this entire period.

We explored these issues by using a comprehensive benefit-cost analytical framework.[2] In Section A, we discuss this approach to benefit-cost analysis and describe how the benefits and costs were calculated. In Section B, we present the results of our benefit-cost analysis of the three treatments. Section C contains a summary and conclusions.

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[1]The decrease in UI payments for the treatment group also caused a slight decrease in tax payments, because the decrease in benefits lowered claimants' incomes.

[2]See Long et al. (1981) for one of the initial applications of this framework.

A. METHODOLOGY

The comprehensive accounting framework that we used to compare the benefits and costs of the three NJUIRDP treatments included several steps. The first step defined the various perspectives from which benefits and costs were measured. We considered the benefits and costs to UI claimants, to determine whether the treatments were beneficial to those whom they were designed to serve. We also considered the perspective of employers who hired claimants, to examine the net effects of their hiring decisions, and the perspective of the government, to assess the budgetary impacts of each treatment relative to existing programs. We also broke the government perspective down into that of individual Labor Department programs (that is, UI, ES, and JTPA) to obtain more insight into the budgetary implication of these treatments.

After the relevant perspectives were defined, the next step in the analysis constructed a comprehensive list of the expected benefits and costs from each perspective.[3] From the perspective of UI claimants, the key benefit of demonstration treatments would be an increase in earnings and fringe benefits generated by more rapid reemployment (and/or higher earnings). More rapid reemployment should also be a psychological benefit to claimants, because most people find unemployment stressful. On the other hand, the increased reporting requirements under the demonstration imposed a cost on claimants, by reducing their time for leisure and nonmarket activities. Other costs to claimants included loss of some UI benefits from more rapid reemployment, additional taxes due on their increased earnings, and any costs from working (for example, child care or transportation expenses).

Employers benefited from the increased output produced by claimants who were hired more rapidly as a result of the treatment, but they also incurred costs, because they had to compensate employees with salaries and fringe benefits. We assumed that the value of the additional output to

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[3]Some of the benefits and costs were difficult to value in dollar terms. Although these "intangible" benefits or costs, such as the psychological benefits to claimants from obtaining a job, are difficult to measure, it is still important that they be assigned to a specific perspective, so that policy judgments can be made about their likelihood of affecting the measured benefit-cost comparisons.

employers equaled the value of the additional compensation by employers, which implies that they incurred no net benefits or costs from these treatments. However, this assumption may understate the benefits derived by employers from a labor market that functions more effectively, which would reduce their recruiting and turnover costs. Alternatively, the treatments might impose a cost on some employers if some temporarily laid-off workers were unavailable for rehire.[4] The Labor Department perspective includes the perspectives of UI and ES, which are funded through the UI payroll tax, and of the JTPA system, which is funded through general revenues. These agencies would incur the costs of providing each of the three treatments in an ongoing program. They would benefit from their direct share of tax increases paid by claimants and their employers, and from reductions in UI benefits paid to claimants and in the costs of providing these benefits. In addition, their costs would be partially offset by a reduction in the costs of providing existing services. Whether the increase in payroll taxes or the reduction in UI benefits was large enough to offset the net costs of the demonstration was one of the key issues in this analysis.

Other sectors of government would inevitably derive net benefits from these treatments (assuming that at least some positive earnings impacts occur), because they receive the portion of claimants' taxes not used to fund Labor Department programs.[5]

The benefits and costs from all of these perspectives were summed to determine the benefits and costs to society as a whole. On the benefit side, the claimants' increased earnings represented an increase in total output and thus a net benefit to society. The assumption underlying this approach to valuing output is that the more rapid reemployment of demonstration claimants did not displace the employment of other individuals. This no-displacement assumption seems reasonable, given the

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[4]Both employers and claimants could also be affected by any changes in taxes resulting from an increase (or reduction) in government costs from offering the treatments. However, any such changes would occur only in the long run, and their effect would depend on how the treatments were funded. For this reason, they are not included in this analysis.

[5]Other sectors of the government could also benefit if the treatments reduced the receipt of such benefits as food stamps or other public assistance. We examined this potential effect but found no impacts, so we did not include these potential impacts in the benefit-cost framework.

strength of the New Jersey economy. On the cost side, the net operational costs of offering each treatment represented social resources that could have been spent otherwise and were measured as net costs to society.

The general approach for valuing the benefits and costs of the three treatments included measuring the market value of the resources consumed, saved, or produced as a result of the treatment, compared with the existing services available to UI claimants. The market value of these resources was estimated for the period in which they were expended or received. We estimated demonstration costs for the period during which the demonstration operated (which corresponded roughly to fiscal year 1987), assuming that all costs were incurred during this period. On the other hand, demonstration benefits could have been realized over a longer period. In our benefit-cost analysis, we allowed for the potential impact of the demonstration on UI benefits and earnings in the years following the demonstration. All these long-term impacts are expressed in terms of their present value during the operational period of the demonstration.[6] The impacts on UI benefits in years after the initial claim year were deflated, using the GNP implicit price deflator, and discounted using a five percent discount rate.[7] Impacts on earnings that occurred after the year of the initial claim were deflated and discounted in the same way.

For the purposes of the benefit-cost analysis, we wanted to measure the net cost of each treatment--using only those costs that would be incurred in an ongoing program--relative to the costs of the UI, ES, and JTPA services that are currently used by the target population. This comparison is based on the principle that claimants in the demonstration treatments received some services that they would have received even in the absence of the demonstration. For example, some claimants

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[6]Actual market prices were used to value benefits and costs whenever available, on the assumption that these prices were the best measure of the true costs of these resources. When market prices were not available, we estimated the dollar value of resources. For example, we estimated the value of fringe benefits, taxes, and the administrative costs of government agencies.

[7]We assumed that all the impacts of the special unemployment compensation programs (EUC and the state programs) occurred in year six.

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referred by the demonstration to JTPA services would have gone to JTPA for services on their own. In order to measure the extent to which the costs of the demonstration services were greater than the costs of providing the existing services, we compared the costs of the demonstration services with the costs of the services received by the control group. Corson et al. (1989) describe the calculations of the costs of the services provided in the demonstration.

B. BENEFITS AND COSTS FROM ALTERNATIVE PERSPECTIVES

In this section, we present estimates of the net benefits and costs of the three treatments relative to the existing services available to UI recipients. As discussed previously, our basic approach in this analysis relied on administrative data on long-term regular UI receipt and interview data on earnings. We then extended our basic approach by using impacts on all unemployment compensation programs and long-term earnings impacts based on the wage records.

1. Benefit-Cost Analysis of JSA-Only Versus Existing Services

The benefits of the JSA-only treatment outweighed the costs from the perspectives of claimants, the Labor Department, the entire government, and society as a whole. Our estimates, which are summarized in Table V.1, revealed that members of the JSA-only group increased their earnings by an average of $608, relative to members of the control group. We imputed another $128 in additional fringe benefits to reach a total increase of $736 in compensation. Much of this increase in compensation benefited claimants, but enough of the increased earnings was returned to the government sector via increased taxes and reduced UI benefits that the government realized a net gain of $175 per claimant. When we examined the UI, ES, and JTPA programs that comprise the Labor Department sector, we found that the savings roughly equaled costs (we calculated a savings of $52, a small increase compared with our previous estimates, due to increased long-run UI savings). The estimated net social gain, which can be taken as an indicator of the efficiency of the treatment, is $581 per claimant.

TABLE V.1

BENEFIT-COST COMPARISON OF THE JSA-ONLY TREATMENT WITH EXISTING SERVICES
(Dollars per Claimant)

Benefits and Costs	Society	Employers	Claimants	Government		
				Labor Dept.	Other Government	Government Total
Market Output and Wages						
Increased output	736	736	0	0	0	0
Wages and fringe benefits	0	-736	736	0	0	0
Tax Payments						
Claimants' taxes	0	0	-129	6	123	129
Income Support Payments						
UI payments	0	0	-200	200	0	200
Other payments	0	0	0	0	0	0
Administrative Costs of Income Support Programs						
UI payment administration	1	0	0	1	0	1
Administration of other programs	0	0	0	0	0	0
Demonstration Costs						
Classroom training costs	0	0	0	0	0	0
On-the-job training costs	0	0	0	0	0	0
Relocation assistance	0	0	0	0	0	0
Reemployment bonuses	0	0	0	0	0	0
Local office labor costs	-118	0	0	-118	0	-118
Central office labor costs	-25	0	0	-25	0	-25
Other costs (direct and indirect)	-26	0	0	-26	0	-26
Offsetting Costs of Existing Services						
ES costs	10	0	0	10	0	10
JTPA costs	-1	0	0	-1	0	-1
UI costs	5	0	0	5	0	5
Sum of Measured Benefits and Costs	581	0	407	52	123	175
Nonmonetary Factors						
Psychological benefits of earlier reemployment	+		+			
Burden of reporting requirements, reduced leisure time, and costs from working	-		-			

NOTE: Row or column sums may not add to the totals because of rounding. UI payments were measured over the six-year period following the claim date for each individual. Payments received in the years after the year of the initial claim were deflated using the GNP implicit price deflator and expressed as a present value (as of the year of the initial claim), based on a discount rate of 5 percent. All other outcomes presented in this table were measured during the year of the initial claim.

2. Benefit-Cost Analysis of the JSA Plus Training or Relocation Treatment Versus Existing Services

The JSA plus training treatment provided net benefits to claimants and generated net costs to the government sector, while society as a whole roughly broke even. The earnings of the JSA plus training claimants were $345 higher, on average, than those of control group members. We imputed an additional $72 of increased fringe benefits, for an increase of $417 in total compensation, as shown in Table V.2. These increased earnings and fringe benefits represented a benefit to claimants, which was partially offset by an increase in taxes of $63 per person and a reduction in UI benefits of $154 per person. The estimated net benefit per claimant for the JSA plus training treatment (compared with existing programs) was $200.

The substantial costs of providing training to the JSA plus training group members who pursued this option, together with the prolonged UI benefits received by trainees while they participated in training, meant that the government sector incurred net costs for the JSA plus training treatment. Members of the treatment group were three to four times more likely to pursue training than were control group members. In addition, they received the same set of initial job-search services from the ES as did those in the JSA-only treatment group. The increased taxes and reduced UI benefits that were generated by the increased employment of persons in the JSA plus training treatment substantially offset the costs of the JSA services, but did not begin to cover the costs of the training itself. The net government loss of $159 per claimant can be broken down into a $219 loss for Labor Department programs, and a $60 gain for the rest of the government.

From the perspective of society as a whole, the choice between the JSA plus training treatment and existing services appears fairly even, with a slight net benefit of $41 per person in favor of the treatment.

Compared with the JSA-only treatment, the benefits from the JSA plus training or relocation treatment were lower than their costs from all perspectives. In particular, JSA plus training or relocation claimants realized lower earnings increases, on average, but the costs of the treatment were

TABLE V.2

BENEFIT-COST COMPARISON OF THE JSA PLUS TRAINING OR RELOCATION ASSISTANCE TREATMENT WITH EXISTING SERVICES
(Dollars per Claimant)

Benefits and Costs	Society	Employers	Claimants	Government		
				Labor Dept.	Other Government	Government Total
Market Output and Wages						
Increased output	417	417	0	0	0	0
Wages and fringe benefits	0	-417	417	0	0	0
Tax Payments						
Claimants' taxes	0	0	-63	3	60	63
Income Support Payments						
UI payments	0	0	-154	154	0	154
Other payments	0	0	0	0	0	0
Administrative Costs of Income Support Programs						
UI payment administration	1	0	0	1	0	1
Administration of other programs	0	0	0	0	0	0
Demonstration Costs						
Classroom training costs	-224	0	0	-224	0	-224
On-the-job training costs	-23	0	0	-23	0	-23
Relocation assistance	-3	0	0	-3	0	-3
Reemployment bonuses	0	0	0	0	0	0
Local office labor costs	-183	0	0	-183	0	-183
Central office labor costs	-29	0	0	-29	0	-29
Other costs (direct and indirect)	-29	0	0	-29	0	-29
Offsetting Costs of Existing Services						
ES costs	10	0	0	10	0	10
JTPA costs	99	0	0	99	0	99
UI costs	5	0	0	5	0	5
Sum of Measured Benefits and Costs	41	0	200	-219	60	-159
Nonmonetary Factors						
Psychological benefits of earlier reemployment	+		+			
Burden of reporting requirements, reduced leisure time, and costs from working	-		-			

NOTE: Row or column sums may not add to the totals because of rounding. UI payments were measured over the six-year period following the claim date for each individual. Payments received in the years after the year of the initial claim were deflated using the GNP implicit price deflator and expressed as a present value (as of the year of the initial claim), based on a discount rate of 5 percent. All other outcomes presented in this table were measured during the year of the initial claim.

much higher. It seems that the most plausible interpretation of this finding is that claimants in the JSA plus training treatment who entered training (or who hoped to enter training) deferred reentering the labor market, and did not increase their earnings capacity sufficiently to compensate for the fewer number of weeks that they worked in the year after the claim date.

3. Benefit-Cost Analysis of the JSA Plus Bonus Treatment Versus Existing Services

Members of the JSA plus reemployment bonus treatment group experienced earnings gains that were similar to those of claimants in the JSA-only group, as well as larger UI benefit reductions than the other two treatment groups, while the costs of this treatment fell between the costs of the other two. On balance, a substantial net gain of $565 per claimant accrued to society, relative to existing services, as shown in Table V.3.

Claimants experienced a net benefit of $400 on average, comprising a $591 increase in earnings and a $124 increase in fringe benefits, balanced by a $314 reduction in UI benefits and a $126 increase in taxes. The government benefited overall from the treatment, and the Labor Department programs experienced a small net gain of $45 per claimant. The rest of the government experienced a net gain of $154 from an increase in taxes.

Overall, the findings for the JSA plus reemployment bonus treatment were similar to those for the JSA-only treatment. The earnings gains experienced by claimants were similar, and while the bonus payments represented a cost to the government sector and a gain to claimants, this cost (and gain) was offset by the larger reduction in UI payments.

4. Alternative Benefit-Cost Estimates

In this section, we recalculate the benefit-cost estimates using unemployment impact estimates for all UI programs as well as earnings impacts based on wage records, rather than on the follow-up interviews. The UI impacts calculated for all programs may overstate UI impacts, because the observed impacts for the temporary programs, particularly EUC, occurred long after the

TABLE V.3

BENEFIT-COST COMPARISON OF THE JSA PLUS REEMPLOYMENT BONUS TREATMENT WITH EXISTING SERVICES
(Dollars per Claimant)

Benefits and Costs	Society	Employers	Claimants	Government		
				Labor Dept.	Other Government	Government Total
Market Output and Wages						
Increased output	715	715	0	0	0	0
Wages and fringe benefits	0	-715	715	0	0	0
Tax Payments						
Claimants' taxes	0	0	-126	6	120	126
Income Support Payments						
UI payments	0	0	-314	314	0	314
Other payments	0	0	0	0	0	0
Administrative Costs of Income Support Programs						
UI payment administration	1	0	0	1	0	1
Administration of other programs	0	0	0	0	0	0
Demonstration Costs						
Classroom training costs	0	0	0	0	0	0
On-the-job training costs	0	0	0	0	0	0
Relocation assistance	0	0	0	0	0	0
Reemployment bonuses	0	0	125	-125	0	-125
Local office labor costs	-118	0	0	-118	0	-118
Central office labor costs	-30	0	0	-30	0	-30
Other costs (direct and indirect)	-26	0	0	-26	0	-26
Offsetting Costs of Existing Services						
ES costs	10	0	0	10	0	10
JTPA costs	8	0	0	8	0	8
UI costs	5	0	0	5	0	5
Sum of Measured Benefits and Costs	565	0	400	45	120	165
Nonmonetary Factors						
Psychological benefits of earlier reemployment	+		+			
Burden of reporting requirements, reduced leisure time, and costs from working	-		-			

NOTE: Row or column sums may not add to the totals because of rounding. UI payments were measured over the six-year period following the claim date for each individual. Payments received in the years after the year of the initial claim were deflated using the GNP implicit price deflator and expressed as a present value (as of the year of the initial claim), based on a discount rate of 5 percent. All other outcomes presented in this table were measured during the year of the initial claim.

demonstration was implemented and hence may not be plausible. In addition, such temporary programs will not necessarily be available in future applications of the New Jersey treatments. The wage-records data may understate earnings impacts because (1) the data do not include the earnings of the self-employed and those who found new employment outside New Jersey, and (2) the data begin with the first calendar quarter after the claim date, and thus miss treatment-control differences for much of the sample in the first month or two after the claim date. On the other hand, if the treatments do have long-term impacts on earnings, we can use the wage-records data to incorporate these long-term impacts into the benefit-cost analysis. The calculations of benefits and costs presented in this section are based on wage records for six years following the claim date. Table V.4 presents the newly calculated net benefit estimates and the benchmark estimates for each treatment.

The estimates that incorporate impacts for all UI programs do not change any of our basic conclusions regarding the benefits and costs of the treatments. These estimates do not affect our societal benefit-cost estimates because they affect only transfers between claimants and the government. In addition, the only treatment showing any substantial change in the estimates was the JSA plus training or relocation treatment. We observed relatively large reductions in UI receipt for EUC in this treatment. However, even after we considered this change, the Labor Department and the government as a whole continue to experience a net loss under this treatment.

The estimates that incorporate our long-run earnings impact estimates affected the benefit-cost analysis of each of the treatments. The long run earnings impact estimates exceeded the one-year estimates used in the alternative benefit-cost estimates for the JSA-only and the JSA plus reemployment bonus treatments. As a result, the positive net benefits increased for claimants, the government, and society under these treatments. The reverse, however, occurred for the JSA plus training or relocation treatment because the long-run earnings impact estimate was reduced. For this treatment, net benefits become negative from all perspectives.

TABLE V.4

SUM OF THE BENEFITS AND COSTS OF ALL TREATMENTS, BASED ON ALTERNATIVE ESTIMATES OF UNEMPLOYMENT COMPENSATION AND EARNINGS IMPACTS
(Dollars Per Claimant)

			Government		
	Society	Claimants	Labor Dept.	Other Government	Government Total
JSA-Only Treatment					
Using regular UI impacts and earnings impacts from interview data[a]	581	407	52	123	175
Using all UI impacts and earnings impacts from interview data[b]	581	383	84	115	199
Using regular UI impacts and earnings impacts from wage-records data[c]	1,063	773	57	233	290
JSA Plus Training Treatment					
Using regular UI impacts and earnings impacts from interview data[a]	41	200	-219	60	-159
Using all UI impacts and earnings impacts from interview data[b]	41	125	-120	36	-84
Using regular UI impacts and earnings impacts from wage-records data[c]	-245	-17	-221	-8	-228
JSA Plus Bonus Treatment					
Using regular UI impacts and earnings impacts from interview data[a]	565	400	45	120	165
Using all UI impacts and earnings impacts from interview data[b]	565	375	76	113	189
Using regular UI impacts and earnings impacts from wage-records data[c]	791	602	47	142	189

[a]Earnings impacts based on interview data were measured over the year of the initial claim. All other measures used in these calculations are defined in Tables V.1-V.3.

[b]All UI impacts include impacts on regular UI, federal, EUC, New Jersey emergency unemployment benefits, and NJ additional benefits for training. All other measures used in these calculations are defined in Tables V.1-V.3.

[c]Earning impacts based on wage records were measured over the six years following the initial claim. All other measures used in these calculations are defined in Tables V.1-V.3.

C. SUMMARY AND CONCLUSIONS

The results of our benchmark benefit-cost analysis suggest that all three treatments offer net benefits to claimants and to society as a whole, relative to existing services. The JSA-only treatment and the JSA plus reemployment bonus treatment also led to net gains for the government sector as a whole and to the Labor Department agencies. On the other hand, the JSA plus training or relocation treatment was expensive for the government sector. These general conclusions changed little when we incorporated impact estimates for all UI programs. However, when we incorporated estimates of long-run earnings impacts, more changes were observed. The positive benefits to society and claimants found for the JSA-only and JSA plus reemployment bonus treatments became larger, and the net benefits to society and claimants for the JSA plus training or relocation treatment became negative.

When we compared the JSA plus training or relocation treatment with the JSA-only treatment, we found that JSA plus training or relocation costs were higher (or benefits were lower) than those for the JSA-only treatment from all viewpoints, because the costs of the service component of the JSA plus training or relocation treatment were higher and its earnings gains were substantially lower. The JSA plus training or relocation treatment would show more benefits if earnings gains could be sustained over several years. However, evidence from the wage records did not show any impact of the treatment on earnings after the demonstration period.

The net benefits and costs of the JSA plus reemployment bonus treatment appeared similar to those of the JSA-only treatment from all perspectives, although the bonus generated higher costs from the government perspective. These findings suggest that the JSA-only and the JSA plus reemployment bonus treatments generated savings in UI benefits and increases in UI taxes that were greater than the cost of the treatments. Our estimates indicate that both the JSA-only treatment and the JSA plus reemployment bonus treatment would lead to modest net benefits for the Labor Department. On the other hand, the costs of the JSA plus training or relocation treatment exceeded

the savings in UI benefits and increased taxes generated by the treatment. Use of this treatment would require either reducing funding for other programs or increasing taxes, because the treatment appeared to create net costs to the government as a whole.

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APPENDIX A

SAMPLES USED TO ESTIMATE THE PROFILING MODELS

This appendix describes the samples that were used to estimate models that predict a claimant's probability of exhausting UI. These models were used to investigate aspects of worker profiling. The models are discussed in Chapter IV.

We estimated the exhaustion probability models using two alternative samples: (1) a relatively large sample (N=3,153), with all of the basic explanatory variables needed to estimate the model (the "records sample"); and (2) a substantially smaller subsample (N=1,541), for which additional explanatory variables were available (the "survey sample").[1] Both samples included a portion of the NJUIRDP noneligible and eligible groups. We removed from both samples those workers who had missing data or who were not permanently separated. Claimants with missing data represented approximately four percent of the full records sample and six percent of the full survey sample (see Figure A.1). Claimants who were not permanently separated represented approximately 11 percent of the records sample and 6 percent of the full survey sample (see Figure A.2).

To represent NJUIRDP eligibles in both samples, we used the demonstration control group members. These individuals did not receive any NJUIRDP treatments, so their rate of UI exhaustion was not affected by the demonstration. All NJUIRDP eligibles--including all control group members--were permanently separated workers (that is, they had no definite recall date and did not seek employment through a union hiring hall).

Within the full population of UI recipients, NJUIRDP eligibles represented 26.6 percent of all claimants (see Figure IV.1) during the period when sample members filed their initial claims. Eligibles constituted approximately 47 percent of the entire records sample and 71 percent of the portion of the records sample used to estimate the models (see Figure A.1). Eligibles also represented 71 percent of the entire survey sample and 89 percent of the portion of the survey sample used to estimate the models (see Figure A.2).

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[1]These additional variables were collected through a survey conducted by Mathematica Policy Research, Inc., in 1988 for the demonstration evaluation.

FIGURE A.1

DISTRIBUTION OF NJUIRDP RECORDS SAMPLE

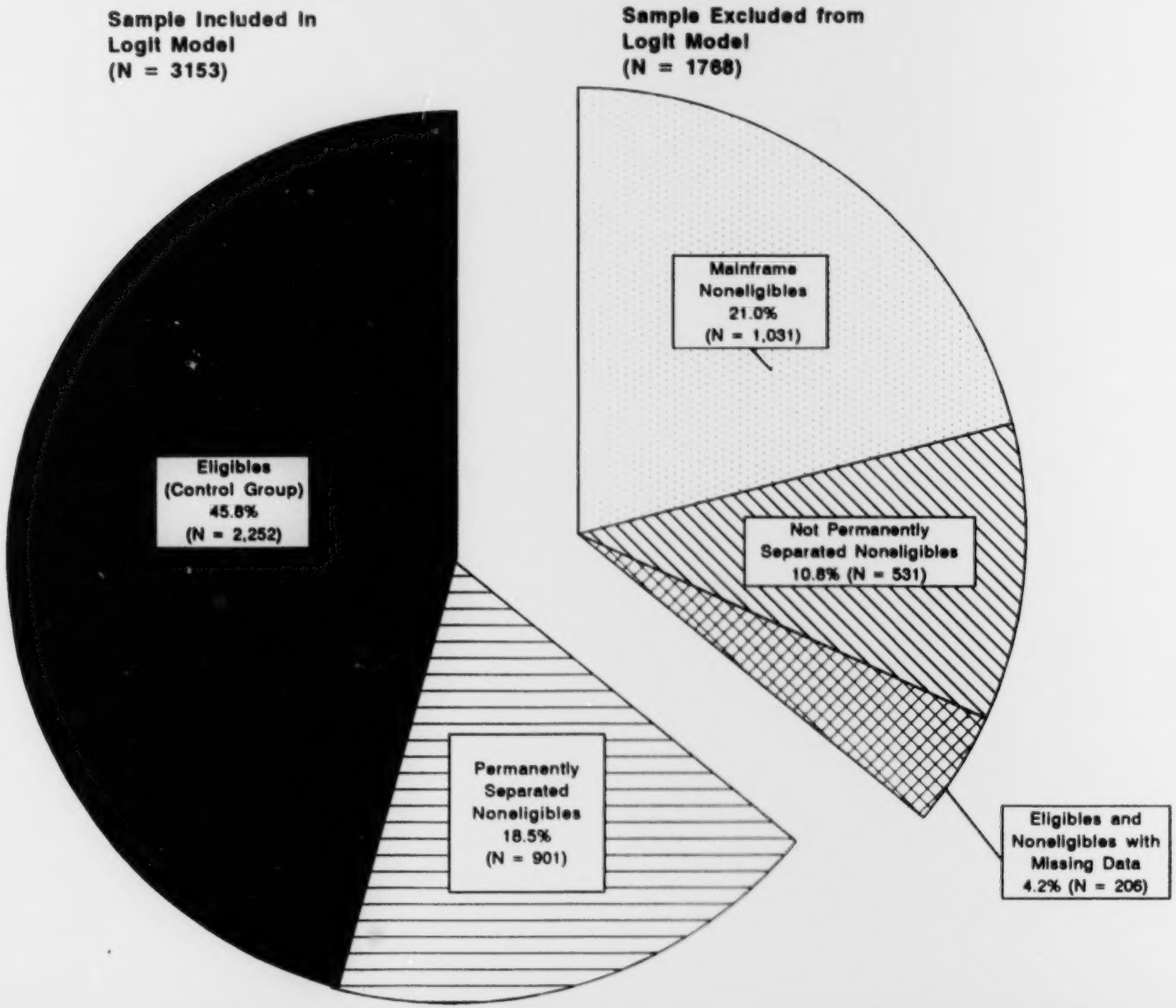
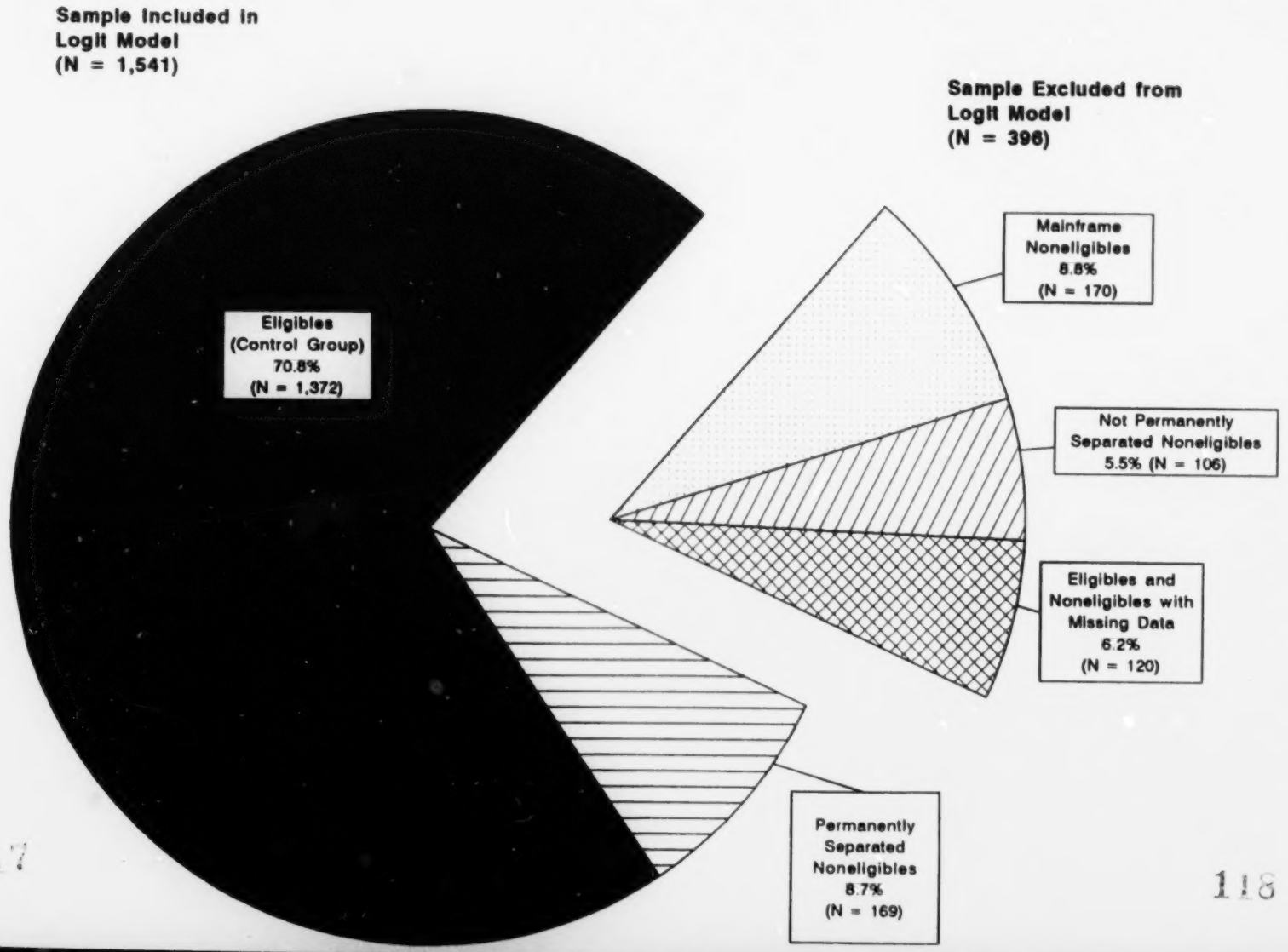

Sample Included in Logit Model (N = 3153)
Sample Excluded from Logit Model (N = 1768)
Eligibles (Control Group) 45.8% (N = 2,252)
Permanently Separated Noneligibles 18.5% (N = 901)
Mainframe Noneligibles 21.0% (N = 1,031)
Not Permanently Separated Noneligibles 10.8% (N = 531)
Eligibles and Noneligibles with Missing Data 4.2% (N = 206)

FIGURE A.2

DISTRIBUTION OF NJUIRDP SURVEY SAMPLE


Sample Included in Logit Model (N = 1,541)
Sample Excluded from Logit Model (N = 396)
Eligibles (Control Group) 70.8% (N = 1,372)
Mainframe Noneligibles 8.8% (N = 170)
Not Permanently Separated Noneligibles 5.5% (N = 106)
Eligibles and Noneligibles with Missing Data 6.2% (N = 120)
Permanently Separated Noneligibles 8.7% (N = 169)

There were two groups of NJUIRDP noneligibles, representing two sets of screens applied sequentially by the demonstration. The first group of workers screened out of the demonstration were the "mainframe noneligibles," workers excluded from NJUIRDP on the basis of data stored on the state's central mainframe computer. The second group of noneligibles, the "Participant Tracking System (PTS) noneligibles," were screened out of NJUIRDP on the basis of data collected in local UI offices and transmitted to the stand-alone computer system used to operate the demonstration's tracking system.

We excluded from the sample all mainframe noneligibles and those PTS noneligibles who were not permanently separated. The mainframe noneligibles were excluded largely because they had incomplete data on some important variables; however, many of these noneligibles would be excluded by an ongoing profiling system.[2]

Workers screened on the basis of their age constituted about 15 percent of all UI claimants. Those excluded because of delays in first payment and positive earnings represented approximately 14 percent and 4 percent of UI claimants, respectively. Approximately 18 percent of all of the mainframe noneligibles were excluded for more than one of these three reasons. Within the full population of UI recipients, the mainframe noneligibles represented 28 percent of all claimants (see Figure IV.1). The mainframe noneligibles represented approximately 21 percent of the full records sample and 9 percent of the survey sample (see Figures A.1 and A.2, respectively).

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[2]In particular, no data relating to recall status and membership in union hiring halls were available for mainframe noneligibles. The NJUIRDP screened out mainframe noneligibles because they (1) received partial payments because they had positive earnings, (2) had a gap between the date of their claim filing and their first payment of more than five weeks, or (3) were younger than age 25. Most states implementing the new profiling policy are expected to exclude workers who received partial payments and had positive earnings because these workers are not permanently separated. Some states may also exclude workers with a five-week or longer gap between their claim date and the date of the first payment, because delays in the first UI payment make it difficult to intervene early in claimants' initial unemployment spells. However, states will not exclude workers under 25 because states are not allowed to use age as a screen for profiling. Workers screened out solely on the basis of age constitute about 10 percent of all UI claimants.

The remaining NJUIRDP noneligibles--the permanently separated PTS noneligibles--were included in the estimation of the model. All of these workers were screened out of the demonstration because they had not worked with their pre-UI employer for three or more years. Within the full population of UI recipients, these nontenured but not permanently separated workers represented approximately 30 percent of all claimants (see Figure IV.1). These workers constituted approximately 18 percent of the entire records sample, 29 percent of the portion of the records sample used to estimate the profiling model, 9 percent of the entire survey sample, and 11 percent of the portion of the survey sample used to estimate the profiling model (see Figures A.1 and A.2).

The actual proportions of record sample members in the various eligible and noneligible groups (see Figure A.1) differ from the proportions shown for the UI population in Chapter IV because the noneligible sample selected for the study was approximately the same size as the control group (the eligibles) rather than three times as large as in the UI population. Similarly the subset of noneligibles included in the survey was an even smaller proportion of the survey sample (see Figure A.2). This underrepresentation of noneligibles may affect our estimates of the effects of claimant characteristics on UI benefit exhaustion particularly for job tenure since sample members with job tenure less than three years come only from the noneligible group.

APPENDIX B

TREATMENT/CONTROL GROUP MEANS

This appendix reports, by treatment and control group, the means and standard deviations of the Unemployment Insurance and employment and earnings outcomes reported in Chapter II. The results of simple difference of means tests for each treatment-control difference are also reported.

The conclusions drawn from this analysis of the UI and employment and earnings impacts of the treatments are the same as those drawn from the regression-adjusted impact estimates presented in Chapter II. That is, while the point estimates of impacts differ slightly, the significant treatment-control group differences reported here are essentially the same as those reported in Chapter II.

TABLE B.1

MEAN UI DOLLARS RECEIVED
(Standard Deviation in Parentheses)

	JSA Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus	Control Group
Year of Initial Claim (First Year)	3,113 ** (1,830)	3,130 ** (1,837)	3,062 *** (1,871)	3,228 (1,797)
Second Year	500 *** (1,255)	562 (1,319)	521 ** (1,265)	600 (1,367)
Third Year	541 (1,391)	544 (1,413)	492 * (1,345)	560 (1,417)
Fourth Year	576 (1,510)	547 (1,490)	565 (1,540)	569 (1,534)
Fifth Year	602 (1,647)	564 (1,607)	626 (1,721)	588 (1,633)
Sixth Year	471 (1,512)	502 (1,558)	436 (1,443)	486 (1,531)
Total	**5,803 (4,781)**	**5,849 (4,877)**	**5,701 ** (4,760)**	**6,031 (4,903)**

NOTE: The sample used for this analysis includes 2,416 JSA only sample members, 3,810 JSA plus training or relocation sample members, 2,449 JSA plus reemployment bonus sample members, and 2,385 control group members.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE B.2

MEAN UI WEEKS PAID
(Standard Deviation in Parentheses)

	JSA Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus	Control Group
Year of Initial Claim (First Year)	17.4 ** (8.78)	17.4 ** (8.71)	17.0 *** (8.83)	17.9 (8.47)
Second Year	2.8 ** (6.64)	3.1 (7.02)	2.9 ** (6.75)	3.3 (7.24)
Third Year	3.0 (7.24)	2.9 (7.08)	2.6 ** (6.81)	3.0 (7.11)
Fourth Year	3.0 (7.35)	2.7 (6.91)	2.8 (7.23)	2.8 (7.05)
Fifth Year	2.8 (7.21)	2.6 (7.02)	2.9 (7.40)	2.7 (7.09)
Sixth Year	2.1 (6.39)	2.2 (6.54)	1.9 (6.11)	2.2 (6.40)
Total	**31.1 (23.4)**	**30.9 (23.2)**	**30.1 *** (22.9)**	**31.9 (23.3)**

NOTE: The sample used for this analysis includes 2,416 JSA only sample members, 3,810 JSA plus training or relocation sample members, 2,449 JSA plus reemployment bonus sample members, and 2,385 control group members.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE B.3

MEAN PROBABILITY OF UI RECEIPT
(Standard Deviation in Parentheses)

Probability of Receipt	JSA Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus	Control Group
Second Year	.198 *	.210	.200	.219
	(.378)	(.407)	(.399)	(.413)
Third Year	.175	.175	.162 *	.183
	(.380)	(.380)	(.369)	(.387)
Fourth Year	.166	.160	.156	.165
	(.372)	(.367)	(.363)	(.371)
Fifth Year	.157	.144	.153	.151
	(.364)	(.351)	(.360)	(.358)
Sixth Year	.115	.122	.108	.122
	(.320)	(.328)	(.311)	(.327)
Total Number of Claims After Initial Claim (Year 2 to Year 6)	**.812** **(1.28)**	**.812** **(1.27)**	**.778 ** ** **(1.24)**	**.840** **(1.29)**

NOTE: The sample used for this analysis includes 2,416 JSA only sample members, 3,810 JSA plus training or relocation sample members, 2,449 JSA plus reemployment bonus sample members, and 2,385 control group members.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE B.4

TOTAL UI DOLLARS RECEIVED THROUGH OCTOBER 1993, BY PROGRAM
(Standard Deviation in Parentheses)

Extended Benefits Program	JSA Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus	Control Group
EUB	44	46	50	48
	(227)	(230)	(253)	(248)
EUC[a]	724	635 **	720	763
	(2,021)	(1,885)	(2,070)	(2,128)
ABT[a]	13	9	12	10
	(247)	(209)	(270)	(198)
Regular UI[b]	5,803	5,849	5,701 **	6,031
	(4,781)	(4,877)	(4,760)	(4,903)
All UI[a]	6,584	6,538 *	6,484 **	6,852
	(6,154)	(6,099)	(6,191)	(6,355)

NOTE: The sample used for this analysis includes 2,416 JSA only sample members, 3,810 JSA plus training or relocation sample members, 2,449 JSA plus reemployment bonus sample members, and 2,385 control group members.

[a]For some sample members, we did not have complete data on these variables for the sixth year following random assignment.

[b]Regular UI impacts are for the first through sixth year after random assignment.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE B.5

TOTAL UI WEEKS PAID THROUGH OCTOBER 1993, BY PROGRAM
(Standard Errors in Parentheses)

Extended Benefits Program	JSA Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus	Control Group
EUB	.24	.24	.25	.24
	(1.17)	(1.18)	(1.21)	(1.19)
EUC[a]	3.51	2.98 **	3.31	3.52
	(9.39)	(8.50)	(9.07)	(9.49)
ABT[a]	.05	.04	.05	.04
	(1.00)	(.82)	(1.06)	(.80)
Regular UI[b]	31.1	30.9	30.1 ***	31.9
	(23.4)	(23.2)	(22.9)	(23.3)
All UI[a]	34.86	34.20 *	33.70 **	35.66
	(29.64)	(28.50)	(28.86)	(29.49)

NOTE: The sample used for this analysis includes 2,416 JSA only sample members, 3,810 JSA plus training or relocation sample members, 2,449 JSA plus reemployment bonus sample members, and 2,385 control group members.

[a]For some sample members, we did not have complete data on these variables for the sixth year following random assignment.

[b]Regular UI impacts are for the first through sixth year after random assignment.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE B.6

PROBABILITY OF WORKING[a]
(Standard Deviation in Parentheses)

Quarter/Year After Claim	JSA Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus	Control Group
Quarter				
1	.50	.50	.53 ***	0.49
	(.50)	(.50)	(.50)	(.50)
2	.59	.57	.59	0.57
	(.49)	(.49)	(.49)	(.50)
3	.64	.63	.63	0.63
	(.48)	(.48)	(.48)	(.48)
4	.63	.62	.62	0.63
	(.48)	(.49)	(.49)	(.48)
Year				
1	.77	.77	.78	.76
	(.42)	(.42)	(.42)	(.43)
2	.73	.72	.72	.73
	(.44)	(.45)	(.45)	(.45)
3	.70	.68	.68	.69
	(.46)	(.47)	(.47)	(.46)
4	.65	.63	.64	.64
	(.48)	(.48)	(.48)	(.48)
5	.61	.59	.60	.59
	(.49)	(.49)	(.49)	(.49)
6	.56	.55	.55	.55
	(.50)	(.50)	(.50)	(.50)
Total (Year 1 to Year 6)	**4.02** (2.27)	**3.97** (2.28)	**3.97** (2.29)	**3.96** (2.28)

NOTE: The sample used for this analysis includes 2,416 JSA only sample members, 3,810 JSA plus training or relocation sample members, 2,449 JSA plus reemployment bonus sample members, and 2,385 control group members.

[a]The probability of working is defined as having reported earnings in a quarter or in a year.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE B.7

MEAN EARNINGS, IN DOLLARS
(Standard Deviation in Parentheses)

Quarter/Year After Claim	JSA Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus	Control Group Mean
Quarter				
1	1,671	1,698	1,838 **	1,638
	(2,904)	(3,122)	(3,142)	(2,934)
2	2,249	2,158	2,280	2,174
	(3,882)	(3,108)	(2,979)	(3,140)
3	2,602	2,564	2,582	2,507
	(3,109)	(3,091)	(3,015)	(2,922)
4	2,547	2,549	2,627	2,517
	(3,014)	(3,128)	(3,364)	(3,061)
Year				
1	9,068	8,970	9,328 *	8,836
	(10,156)	(10,031)	(10,193)	(9,905)
2	11,535	11,114	11,367	11,253
	(12,549)	(12,010)	(12,416)	(12,405)
3	11,989	11,846	11,990	11,831
	(13,674)	(13,757)	(13,488)	(13,460)
4	11,891	11,753	12,276	11,679
	(13,891)	(14,115)	(14,716)	(13,954)
5	11,806	11,604	11,702	11,657
	(15,074)	(14,897)	(14,637)	(15,251)
6	11,426	11,496	11,555	11,188
	(15,346)	(15,654)	(15,264)	(15,146)
Total	**67,717**	**66,789**	**68,218**	**66,434**
(Year 1 to Year 6)	**(70,167)**	**(69,918)**	**(69,751)**	**(69,013)**

NOTE: The sample used for this analysis includes 2,416 JSA only sample members, 3,810 JSA plus training or relocation sample members, 2,449 JSA plus reemployment bonus sample members, and 2,385 control group members.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE B.8

MEAN WEEKS WORKED
(Standard Deviation in Parentheses)

Quarter/Year After Claim	JSA Only	JSA Plus Training or Relocation	JSA Plus Reemployment Bonus	Control Group
Quarter				
1	4.1	4.0	4.3 ***	3.9
	(5.0)	(5.0)	(5.1)	(4.9)
2	5.8	5.6	5.9 *	5.6
	(5.7)	(5.7)	(5.7)	(5.8)
3	6.9	6.8	6.8	6.7
	(5.8)	(5.8)	(5.8)	(5.8)
4	6.7	6.6	6.7	6.6
	(5.9)	(5.9)	(5.9)	(5.8)
Year				
1	23.5	23.0	23.7 *	22.8
	(18.3)	(18.4)	(18.5)	(18.2)
2	28.9	28.0	28.2	27.6
	(22.5)	(22.1)	(22.2)	(20.8)
3	27.9	27.3	27.7	26.8
	(23.0)	(23.1)	(23.3)	(21.9)
4	26.3	25.7	26.3	24.9
	(23.4)	(23.3)	(23.7)	(22.4)
5	24.4	23.9	23.9	22.9
	(23.6)	(22.5)	(23.5)	(22.5)
6	22.5	22.2	22.3	21.3
	(23.6)	(23.4)	(23.4)	(22.5)
Total	**150.1**	**147.0**	**149.0**	**146.3**
(Year 1 to Year 6)	**(107.5)**	**(108.8)**	**(109.6)**	**(107.5)**

NOTE: The sample used for this analysis includes 2,416 JSA only sample members, 3,810 JSA plus training or relocation sample members, 2,449 JSA plus reemployment bonus sample members, and 2,385 control group members.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

PART II

THE NEW JERSEY UNEMPLOYMENT INSURANCE REEMPLOYMENT DEMONSTRATION PROJECT

SUMMARY AND POLICY IMPLICATIONS

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INTRODUCTION AND SUMMARY

The New Jersey Unemployment Insurance Reemployment Demonstration Project (NJUIRDP) was initiated by the United States Department of Labor (USDOL) through a cooperative agreement with the New Jersey Department of Labor (NJDOL) to test whether the Unemployment Insurance (UI) system could be used to identify displaced workers early in their unemployment spells. The project also tested alternative early intervention strategies to accelerate these individuals' return to work. Three packages of services, or treatments, were tested: (1) job-search assistance (JSA) only; (2) JSA combined with training or relocation assistance; and (3) JSA combined with a cash bonus for early reemployment. A key component of the demonstration was that eligible claimants were identified and services were provided through the coordinated efforts of the Unemployment Insurance (UI), Employment Service (ES), and Job Training Partnership Act (JTPA) systems. Another key component was that UI required claimants to report for services; failure to report could lead to the denial of benefits.

The demonstration began operations in July 1986. By the end of sample selection in June 1987, 8,675 UI claimants were offered one of the three packages. Another 2,385 claimants, who received existing services, were randomly selected to provide a control group for comparative purposes. Services to eligible claimants continued into fall 1987 to ensure that all eligibles, if they desired, were able to receive the full set of demonstration services.

The initial evaluation of the demonstration (Corson et al. 1989), combined with two follow-up studies that extended the analysis for approximately six years after the initial UI claim (Anderson et al. 1991; and Corson and Haimson 1994), found that each treatment reduced UI collections for two or more years and increased employment and earnings for at least the initial year. Although the initial evaluation found no evidence that the training component of the second treatment increased earnings in the year after the initial UI claim, the follow-up studies suggested that training did increase earnings in the longer run. More generally, the follow-up studies suggested that each

component of the treatments--JSA, training, and the reemployment bonus--probably contributed to the impacts on UI receipt and earnings and that the treatments generated more stable jobs than those found by control group members. The evaluation also indicated that the demonstration succeeded in targeting claimants who, in the absence of the demonstration, would have experienced more severe long-run reemployment difficulties. Finally, the evaluation found that all three treatments offered net benefits to claimants and to society, when compared with existing services. The JSA-only and JSA plus reemployment bonus treatments also led to net gains for the government.

These findings suggest that the demonstration treatments represent useful reemployment policies that can be directed toward UI claimants. However, before replicating these policies, it is important to consider several other evaluation findings. First, in addition to the reemployment services themselves, two aspects of the treatments--the participation requirements and the high degree of interagency coordination--appeared to contribute to the success of the treatments. These aspects of the treatments should not be ignored in future applications. Second, analyses of the impacts of the treatments by population subgroup suggest that the treatments were most successful in promoting reemployment for individuals with marketable skills. Finally, benefit-cost analyses of the individual treatments provide the strongest support for the job-search assistance only treatment. These latter two findings suggest that the mandatory job-search assistance services emphasized in the New Jersey demonstration are appropriate and cost-effective for a broad range of permanently separated UI claimants. However, longer-run, more intensive services are probably needed for displaced workers who suffer major structural dislocations.

BACKGROUND AND CURRENT POLICY ENVIRONMENT

The UI system provides short-term income support to involuntarily unemployed individuals while they seek work. The system has historically attempted to promote rapid reemployment by imposing various work-search requirements on claimants and by referring them to the ES for job search assistance, counseling and other services and, through the ES, to training and other services offered

under JTPA or its predecessors. However, observers have noted that the links to reemployment services are often weak and that more intensive services could be provided to help UI claimants become reemployed.[1] It has also been suggested that the more intensive reemployment assistance should target permanently separated or displaced claimants, who are expected to experience the greatest difficulty in becoming reemployed. It has also been argued that, if reemployment assistance were provided early in the UI claim period, the savings in UI benefit payments could potentially outweigh the costs of providing these services. In addition, even if paying for reemployment services for these workers does not prove cost-effective from the standpoint of UI, the UI system may provide important benefits by identifying a broad population of displaced workers *early* in their unemployment spells who could benefit from receiving the services.

Concerns of this nature have led USDOL to sponsor several demonstrations testing the efficacy of reemployment services for displaced UI claimants. In addition to the NJUIRDP, USDOL sponsored the Charleston, South Carolina, Claimant Placement and Work Test Demonstration (Corson et al. 1985), which tested strict enforcement of the ES registration requirement, combined with an increase in the intensity of reemployment services. Another demonstration in Washington State--the Washington Alternative Work Search Experiment (Johnson and Klepinger 1991)--tested alternative work-search requirements combined with more intensive JSA. These demonstrations, as well as several additional ones sponsored by individual states, suggested that increased levels of mandatory reemployment services combined, in some cases, with job-search requirements were likely to promote more rapid reemployment among UI claimants. Further, these demonstrations showed that long-term UI recipients could be identified early in their unemployment spells.

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[1]See, for example, discussions in the National Commission on Unemployment Compensation's final report (National Commission on Unemployment Compensation 1980), the 1988 Secretary of Labor's seminar on alternative uses of unemployment insurance (USDOL 1989), and a recent review of reemployment services by USDOL (USDOL 1994a). See also Richardson et al. 1989 for evidence that few UI claimants, even long-term ones, receive reemployment services.

The evidence from these demonstrations, particularly the New Jersey one, led to recent legislation (the Unemployment Compensation Amendments of 1993) requiring state UI programs to *profile* claimants as they enter the UI system, to identify displaced workers. Subsequent interpretation of this requirement by USDOL provides guidance to states about how to implement profiling (USDOL 1994b). Specifically, states are encouraged to use and adapt a profiling model developed by USDOL. This approach uses a two-step process to identify displaced workers. In the first step, claimants who are permanently separated from their pre-UI jobs are identified; in the second, a probability of exhaustion is estimated for each claimant. Those with the highest probabilities of exhaustion are identified as the target group. States that do not have sufficient data to estimate such models are expected to use a set of screens to identify displaced workers (as in the New Jersey demonstration), but they are encouraged to develop profiling models as more data become available.

Identifying displaced workers is the first step in helping them become reemployed; strengthening linkages to reemployment services is the second step. For this reason, the worker profiling legislation requires state UI systems to refer profiled claimants to reemployment services to the extent possible given resource constraints. These claimants are then expected to participate in reemployment services as a condition of eligibility for UI, unless they have already completed these services or have a justifiable reason for their failure to participate.

To implement these requirements, states are expected to establish agreements between the UI system and service providers (the ES or Economic Dislocation and Worker Adjustment Act--EDWAA--programs), so that profiled claimants can be referred to a service provider and receive services.[2,3] Service providers in each locality are expected to hold initial orientation sessions with claimants, followed by assessment sessions in which individual service plans are developed for each claimant. Participation in the reemployment services outlined in the plans is a condition for

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[2]USDOL modeled this design, in part, on the basic JSA treatment used in the New Jersey demonstration.

[3]The EDWAA program operates as Title III of JTPA.

continued UI eligibility. In addition to orientation and assessment, reemployment services include counseling, job-search assistance (such as job-search workshops), referrals to jobs and job placement, and other similar services. However, these services do not include training or education. Although service providers can refer claimants to training or educational services, and claimants who participate do not have to take part in other reemployment services, participation in training or education is not mandatory. To allow UI monitoring of the participation requirement, states are expected to develop mechanisms to give UI feedback about whether referred claimants take part in required services.

These worker profiling and reemployment service systems, which are currently being developed, are an integral part of recently proposed institutional changes. For example, the Reemployment Act of 1994, which was introduced in Congress in spring 1994, aimed to consolidate existing reemployment services for displaced workers and to provide services in a one-stop-shopping career center. It also attempted to provide additional funds for services, to help states achieve the goal of providing reemployment options to workers. Other proposals to consolidate existing services have also been proposed.

DEMONSTRATION DESIGN

The New Jersey demonstration was designed to address three objectives:

1. Examining the extent to which UI claimants who could benefit from reemployment services could be identified early in their unemployment spells
2. Assessing effective policies and adjustment strategies for helping such workers become reemployed
3. Examining how such a UI reemployment program should be implemented

To achieve these objectives, the design called for identifying demonstration-eligible individuals in the week after their first UI payment, and then assigning them randomly to one of three treatment groups offered alternative packages of reemployment services or to a control group receiving existing services. The demonstration was implemented in 10 sites that corresponded to state UI offices. The

sites were chosen randomly, with the probability of their selection proportional to the size of the UI population in each office.

Definition of Eligibility

The purpose of the demonstration was to provide reemployment services to experienced workers who, having become unemployed through no fault of their own, were likely to face prolonged spells of unemployment. Their job-finding difficulties might be due to unavailability of jobs, a mismatch between their skills and job requirements, or their lack of job-finding skills. Because previous research efforts had not established good predictors of prolonged unemployment spells, complex eligibility requirements could not be used to direct demonstration services. As a result, the demonstration plan incorporated a small number of screens to identify experienced workers who were likely to be displaced permanently from their jobs. The following eligibility screens were chosen:

- ***First Payment***. The demonstration excluded claimants who did not receive a first UI payment. To promote early intervention, it also excluded claimants who did not receive a first payment within five weeks after the initial claim. Individuals who were working and, consequently, received a partial first payment were also excluded, because their job attachment meant that they had not necessarily been displaced. Finally, special claims (for example, unemployment compensation for ex-service members or federal civilian employees, interstate claims, and combined wage claims) were also excluded.

- ***Age***. An age screen was applied to eliminate the broad category of young workers, who have traditionally shown limited attachment to the labor market and whose employment problems may be quite different from those of older, experienced workers. This screen excluded workers under age 25.

- ***Tenure***. Demonstration-eligible claimants had to exhibit a substantial attachment to a job, so that the job loss was likely to be associated with one or more of the reemployment difficulties described earlier. Each claimant was required to have worked for his or her last employer for three years prior to applying for UI benefits and could not have worked *full-time* for any other employer during the three-year period. USDOL's Bureau of Labor Statistics had used the three-year requirement to define displaced workers (Flaim and Sehgal 1985).

- ***Temporary Layoffs***. Because the demonstration treatments were not intended for workers who were temporarily laid off, it was desirable to exclude claimants on temporary layoff. However, previous research and experience show that some claimants say that they expect to be recalled, even when their chances of actual recall are slim. To ensure that these individuals were not excluded from the demonstration, only individuals who both expected to be recalled and had a *specific* recall date were excluded.

- ***Union Hiring-Hall Arrangements.*** Individuals who are typically hired through union hiring halls exhibit a unique attachment to a specific labor market and were thus excluded from the demonstration.

The Treatments

As stated, the demonstration tested three treatment packages for enhancing reemployment. Eligible claimants were assigned randomly to a control group that received existing services or to one of the three treatment groups: (1) JSA only; (2) JSA plus training or relocation; or (3) JSA plus a reemployment bonus.

The initial components of all three treatments were the same: notification, orientation, testing, a job-search workshop, and an assessment/counseling interview. These services were delivered sequentially, early in claimants' unemployment spells. First, a notification letter was sent to claimants in about the fourth week after they filed the initial claim. Claimants usually began to receive services during their fifth week of unemployment. Services began when they reported to a demonstration office (usually an ES office) and received orientation and testing during a one-week period. In the following week, they attended a job-search workshop, consisting of five half-day sessions, and a follow-up, one-on-one counseling/assessment session in the subsequent week. These initial treatment components were mandatory; failure to report could lead to denial of UI benefits.

Beginning with the counseling/assessment interview, the nature of the three treatments differed. In the JSA-only group, claimants were told that, as long as they continued to collect UI, they were expected to maintain periodic contact with the demonstration office, either directly with staff to discuss their job-search activities or by engaging in search-related activities at a resource center in the office. The resource center offered job-search materials and equipment, such as job listings, telephones, and occupational and training literature. Claimants were encouraged to use the center actively and were told that, if they did not come to the office periodically, ES staff would contact them and ask them to do so. These periodic follow-up contacts were to occur at 2, 4, 8, 12, and 16

weeks following the assessment interview. ES staff were expected to notify UI when a claimant did not report for services.

Claimants in the second treatment group--JSA plus training or relocation--were also informed about the resource center and their obligation to maintain contact during their job search. In addition, they were told about the availability of classroom and on-the-job training and were encouraged to pursue training if interested. Staff from the local JTPA Service Delivery Area (SDA) program operator worked directly with these claimants to develop training options. These claimants were also told about the availability of relocation assistance, which those who elected not to pursue training could use for out-of-area job searches and moving expenses.

Claimants in the third treatment group--JSA plus a reemployment bonus--were offered the same set of JSA services as the first group, in addition to a bonus for rapid reemployment. The maximum bonus equaled one-half of the claimant's remaining UI entitlement at the time of the assessment interview. This amount was available if the claimant started working either during the assessment week or in the next two weeks. Thereafter, the potential bonus declined at a rate of 10 percent of the original amount per week, until it was no longer available. Claimants recalled by their former employer could not receive a bonus; neither could those who were employed by a relative or in temporary, seasonal, or part-time jobs. Claimants who collected a bonus received 60 percent of the bonus if they were employed for 4 weeks, and the remainder if they were employed for 12 weeks.

Each treatment tested a different concept of the employment problems displaced workers face. The JSA-only treatment was based on the assumption that many displaced workers have marketable skills but do not have enough job-search experience to identify these skills and sell them in the job market. In contrast, the training treatment was based on the assumption that some workers' skills are outmoded and must be upgraded. Finally, the reemployment bonus treatment was based on the assumption that JSA alone is an insufficient incentive for claimants to obtain employment rapidly, and

that an additional incentive will help them recognize the realities of the job market and accept a suitable job more quickly.

With the exception of the reemployment bonus and relocation assistance, the demonstration services were similar to those available under the existing ES and JTPA systems in New Jersey. However, there were important differences. The likelihood that a claimant was offered and received demonstration services was considerably greater than that under the existing system. The timing of service receipt also differed: demonstration services were generally provided earlier in the unemployment spell. In addition, the mandatory nature of the initial services differed. Under the existing system, non-job-attached claimants were expected to register with the ES, but registration was sometimes delayed during peak load periods and subsequent services were not generally mandatory. Under the demonstration, claimants were expected to report for initial services, and this requirement was enforced.

Provision of Demonstration Services

An important objective of the demonstration was to examine how a reemployment program targeted toward UI claimants should be implemented. The demonstration design emphasized two aspects of this objective: (1) using existing agencies and vendors to provide services; and (2) using a computer-based participant tracking system to facilitate service delivery.

In the New Jersey demonstration, the UI agency, ES, and local JTPA program operators were all involved in delivering services. Strengthening linkages among these agencies was an important component of the demonstration. UI staff were responsible for collecting the data used to select eligible claimants and for monitoring claimants' compliance with the demonstration's reporting requirements. A determination of UI eligibility was made after claimants did not report for the initial mandatory services; if appropriate, benefits were denied.

A four-person team in each demonstration office provided the initial reemployment services, together with additional services offered at the assessment/counseling interview. This team consisted

of three ES staff members and a JTPA staff member from the local SDA program operator. An ES counselor served as team leader and had overall responsibility for ensuring that services were provided. ES staff provided all services for the JSA-only and JSA plus reemployment bonus treatment group members. JTPA staff members were involved only with the JSA plus training/relocation treatment group members. They were expected to be involved with claimants during the assessment/counseling interview, to work with individuals who were interested in classroom or on-the-job training, and to identify appropriate opportunities and place claimants in them. Because the goal was to use training opportunities available in each local JTPA SDA, this component strengthened linkages between the ES and the local JTPA program operators in the 10 demonstration sites.

A computer-based tracking system was used to operate the program and to provide some of the data used for the evaluation. Data on service delivery were entered into the system, and local office staff received lists of claimants each week who were expected to receive services. A list of claimants who did not report for services was also generated for UI, and monitoring reports were sent to central office staff. The system helped ensure that services were delivered as specified, and that claimants were not "lost" from the program.

Economic Environment

During the demonstration period, the New Jersey economy experienced worker displacement caused by a long-term secular decline in manufacturing, although substantial growth occurred in other sectors. Overall, the state economy was quite strong, and the unemployment rate during the demonstration period was low (five percent). The unemployment rate continued to be low (five percent or less) during the first several years of the follow-up period. With the onset of the recent recession, however, it rose in the last two to three years of the follow-up period to between 6.6 and 8.4 percent, on an annual basis. During this latter period, unemployment compensation benefits were

also extended. This extension probably had an effect on UI benefit receipt. As a result, it could have affected our impact estimates for this period.

FINDINGS

Effectiveness of the Eligibility Definition

The eligibility requirements targeted demonstration services to about one-quarter of the claimants who received a first UI payment. The first round of exclusions (for delayed first payments, partial first payments, special claims, and age under 25) was made on the basis of routinely collected UI agency data and an examination of the records of all claimants who received a first payment. This process excluded about 28 percent of the claimants, with the largest number being excluded by the age restriction.

The rest of the eligibility screens (for job tenure less than three years, temporary layoffs, and union hiring-hall arrangements) were implemented with data collected by UI staff specifically for the demonstration. The most restrictive screen applied at this point was the tenure requirement, which excluded individuals who had not worked for their pre-UI employer for three years. This requirement excluded about half the claimants who passed the initial eligibility screens.

The other important eligibility requirement that merits discussion is the temporary layoff screen, which excluded claimants with a definite recall date. This screen excluded about 13 percent of the claimants who survived the initial examination of agency data. In devising this screen, those designing the demonstration decided it was important to establish that the layoff was indeed temporary, rather than relying solely on the claimant's expectation. Having a definite recall date was used for this purpose. As expected, the percentage of claimants who said that their layoff was temporary was substantially larger than the number who actually had a recall date. About half of the claimants who expected to be recalled but who had no recall date did return to their pre-UI job.

The eligibility definition was designed to identify claimants who, in the absence of demonstration services, would experience difficulty in becoming reemployed. An examination of the characteristics

of the eligible population showed that it contained a substantial proportion of individuals whose age, industry of employment, and other characteristics are usually associated with the displaced worker population and with difficulties in becoming reemployed. Moreover, compared with a sample of individuals who were not eligible for the demonstration, the eligible population experienced considerably longer periods of UI collection and longer unemployment spells, on average, during the initial benefit year. During the full six years of followup, the group targeted in the New Jersey demonstration continued to experience large reductions in earnings relative to their base-year earnings. These earnings reductions were considerably larger than those realized by noneligibles. However, the long-term UI receipt of demonstration eligibles was significantly shorter than that of noneligibles. This finding can be attributed to the fact that workers in seasonal industries were among the noneligible population.

These findings indicate that the eligibility screens directed demonstration services to a population that generally faced reemployment difficulties. However, it is unlikely that all demonstration eligibles required services. Some were in the prime of their working lives and some were individuals from industries (for example, the service industry) that were strong and growing in New Jersey. Moreover, some were recalled by their pre-UI employers.

Based in part on the design and the initial findings from the New Jersey demonstration, the Unemployment Compensation Amendments of 1993 mandated that states identify workers likely to exhaust UI and refer them to reemployment services. USDOL has suggested that this targeting process, known as "worker profiling," can occur in a number of ways. One of the principal options involves eliminating workers who are not permanently separated, estimating each individual's probability of exhausting UI, and serving those with the largest predicted probabilities of exhaustion.

In recent simulations of this targeting process that replicated current funding levels, we found that the group that would have been targeted by profiling experienced somewhat greater reemployment problems than the New Jersey eligibles, as reflected in both groups' employment and

UI receipt. These differences were apparent not only in the year following their initial claims but also during the full six years of followup. On the other hand, the group that would be served under profiling experienced smaller earnings reductions, relative to pre-UI earnings, than did the New Jersey demonstration eligibles. This finding is probably due to the fact that the targeted group had fewer years of job tenure on average than the New Jersey demonstration eligibles. As a result they had lower average pre-UI earnings and suffered smaller earnings reductions.

Receipt of Initial Services

All claimants who were selected as demonstration treatment group members were offered a common set of reemployment services early in their UI claim period. These services occurred in sequence and consisted of orientation, testing, a job-search workshop, and an assessment/counseling interview.

Data on the receipt of these initial services show that 77 percent of the selected claimants attended orientation as requested (see Table 1). Most attended their scheduled session, but some attended a later session, generally after follow-up contact by the UI claims examiner. Three-quarters of the claimants who attended orientation continued through the assessment/counseling interview. However, not all such individuals were tested or attended a job-search workshop. Some individuals were excused from all services, generally because their recall expectations could be substantiated. In addition, a large number were excused from testing and the workshop because of language or reading comprehension difficulties that precluded testing. This situation suggests that programs might want to emphasize referrals to English as a Second Language courses or remedial education for such individuals, as part of the early orientation and screening process.

Most claimants attended orientation during the fifth week after their UI claim, and most completed assessment during the next three- to four-week period. Thus, the goal of early intervention was achieved as planned.

The level at which treatment group members received the initial services--testing, job-search workshops, and counseling--substantially exceeded the level at which control group members received such services from ES and JTPA through existing referral mechanisms. Thus, the demonstration achieved its objective of increasing the level of services eligible claimants received.

Receipt of Additional Services

The additional services that were offered to claimants at the assessment/counseling interview included the periodic JSA activities, training and relocation assistance, and the reemployment bonus.

JSA Followup. The objective of the follow-up activities was to encourage all claimants, except those in the second treatment who were engaged in training, to pursue ongoing, intensive job search. This intensive job search was promoted by disseminating job-search materials at the resource centers and by requiring claimants to maintain periodic contact with demonstration staff, either through the resource centers or directly, in person.

Data on claimants who were collecting UI at the five follow-up points (2, 4, 8, 12, and 16 weeks after assessment) showed that 92 percent satisfied the first follow-up requirement (that is, the 2-week contact), and 80 percent had a contact at 16 weeks. Although the rate of contact declined somewhat at the later contact points, the degree of contact was high relative to ongoing employment and training programs, which typically do not have systematic follow-up procedures. However, these periodic contacts did not always follow the strict schedule that had been laid out in the design, nor were all the contacts made in person as desired. In addition, only a few of the resource centers appear to have been used fairly extensively; consequently, the use of these centers probably had a minor impact, at most, on demonstration outcomes.

TABLE 1

RECEIPT OF INITIAL REEMPLOYMENT SERVICES

	Total
As Percentage of the Total Sample	
Attended Orientation	
Scheduled orientation	67.9
Later orientation	8.9
Total	76.8
Tested	45.5
Excused from Testing[a]	28.4
Completed Job-Search Workshop[b]	49.8
Excused from Job-Search Workshop	19.8
Attended Assessment/Counseling Interview	56.2
As Percentage of Those Attending Orientation	
Tested	59.2
Excused from Testing	37.0
Completed Job-Search Workshop	64.8
Excused from Job-Search Workshop	25.8
Attended Assessment/Counseling Interview	73.2
Sample Size	8,675

[a]Includes 0.2 percent who were excused because they had previously been tested by the ES.

[b]Includes 0.5 percent who were excused because they had already completed a job-search workshop.

Training and Relocation Assistance. Classroom and on-the-job training opportunities were offered to claimants in the second treatment to test the efficacy of a service package that, early in the unemployment spell, attempted to alter or upgrade skills no longer in demand.[4] About 15 percent of the claimants who were offered training participated in it. Most of this training was classroom based. Much of the classroom training was in business and office services or computer and information services, while the on-the-job training tended to be in technical, clerical, and sales occupations. It appears that the training offered was directed toward occupations with strong employment prospects in New Jersey.

The rate of training receipt was higher than the rate observed for comparable groups of claimants who were offered training opportunities through referrals to the regular JTPA service environment in New Jersey. Thus, the offer of training under the demonstration achieved the objective of increasing the receipt of training. Nevertheless, the overall rate of training receipt was lower than initially expected.

Two general reasons appear to explain the lower-than-expected increase in training participation. First, the nature of the training intervention differed from that offered by other programs. The offer occurred early in the layoff period, which may have been before many individuals were ready to accept the fact that an occupational change was necessary. Moreover, not all individuals who were offered training were interested in or needed reemployment services, let alone training. However, they were offered services because of the mandatory nature of the initial services.

The second reason that training participation was lower than might have been expected pertains to the demonstration implementation. The training treatment relied on existing JTPA program operators to provide the training placement function, and some operators were considerably more successful than others at placing claimants in training. Their success stemmed from a number of

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[4]Individuals in this treatment group were also offered relocation assistance. As previous experience has suggested, few individuals were interested in relocation, and fewer than one percent of those who were offered relocation assistance received it.

factors, including an early and enthusiastic presentation of the training option and the capability to offer a wide range of individual training slots.

Reemployment Bonus. The third treatment package included a reemployment bonus that was offered to claimants at the assessment/counseling interview. The purpose was to provide a direct financial incentive for claimants to seek work actively and become reemployed. The full bonus offer averaged $1,644 and was paid for jobs that started by the end of the second full week following the interview. After that point, it declined by 10 percent of the initial amount each week, so that it fell to zero by the end of the 11th full week of the offer.

Nineteen percent of the claimants who were offered the bonus received a first bonus payment, which was paid to individuals who held a bonus-eligible job for at least four weeks. Eighty-four percent of this group also received the final bonus payment, which was paid after 12 weeks of work. Overall, the total of the two bonus payments paid averaged close to $1,300.

About 30 percent of the claimants who were offered a bonus began a job within the bonus period, compared with the 19 percent who received a bonus. The remaining 12 percent appeared largely ineligible for the bonus, primarily because they obtained a job with their pre-UI employer (claimants who returned to their pre-UI employers were not eligible).

Impacts of the Demonstration Treatments on UI Receipt

The demonstration treatments were expected to affect the receipt of UI benefits by eligible claimants. The JSA-only and JSA plus reemployment bonus treatments (the first and third treatments) were expected to help eligible claimants become reemployed rapidly, thereby reducing the amount of UI benefits received by treatment group members, relative to the amount received by control group members. Further, the JSA plus reemployment bonus treatment was expected to have a larger impact on UI receipt because of the reemployment incentives created by the bonus. Expectations about the JSA plus training or relocation treatment on short-run UI receipt were less clear. Individuals who received this treatment but not training were expected to experience a

reduction in UI receipt, but those who entered training were expected to experience an increase in receipt, since individuals who accepted training continued to collect UI.

Estimates of the treatment impacts on regular UI receipt show that all three treatments reduced weeks collected over the benefit year, by a half week for the first two treatments and a week for the third (see Table 2). As expected, these reductions were largest for the third treatment--JSA plus the reemployment bonus. As shown in the table, these impacts were mirrored in the amount of benefits collected.

Somewhat surprisingly, longer-run reductions in UI receipt were also observed. Significant reductions occurred in the second year for the JSA-only and JSA plus reemployment bonus treatments. In addition, a significant reduction in extended benefit program payments occurred for the JSA plus training or relocation assistance treatment.[5] For all UI programs during the six-year follow-up period, the treatments reduced UI benefit receipt by about three-quarters of a week for the JSA-only treatment, by one and a half weeks for the JSA plus training or relocation assistance treatment, and by nearly two weeks for the JSA plus reemployment bonus treatment. These findings suggest that each of the treatment components--JSA, training, and the reemployment bonus--probably contributed to the longer-term impacts and that the treatments, in general, generated jobs that were more stable than those found by control group members.

Employment and Earnings Impacts

In general, the treatments were expected to promote the rapid reemployment of claimants, thus having a positive impact on the employment and earnings of claimants after their entry into the UI system. As noted, short-run impacts were expected to be greater for the JSA-only and JSA plus reemployment bonus treatments than for the JSA plus training treatment, since individuals who entered training were expected to sacrifice short-run earnings for longer-run earnings gains.

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[5]Specifically the reduction was in Emergency Unemployment Compensation (EUC) benefits.

TABLE 2

ESTIMATED TREATMENT IMPACTS ON UI RECEIPT

	JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus	Control Group Mean
Regular UI				
Weeks Paid in Benefit Year	-0.47 *	-.048 **	-0.97 ***	17.9
Weeks Paid in Second Year	-0.53 ***	-0.22	-0.44 **	3.3
Weeks Paid Over Six Years	-0.76	-0.93	-1.72 ***	31.9
Dollars Paid in Benefit Year	-87 *	-81 *	-170 ***	3,228
Dollars Paid in Second Year	-94 ***	-39	-78 **	600
Dollars Paid Over Six Years	-181	-165	-333 *	6,031
All UI Programs[a]				
Weeks Paid Over Six Years	-0.78	-1.47 **	-1.92 **	35.7
Dollars Paid Over Six Years	-222	-293 *	-375 **	6,852

[a]Includes regular UI, Emergency Unemployment Compensation (EUC) and two special state extended benefit programs.

* Statistically significant at the 90-percent confidence level for a two-tailed test.
** Statistically significant at the 95-percent confidence level for a two-tailed test.
*** Statistically significant at the 99-percent confidence level for a two-tailed test.

Estimates of the short-run impacts of the treatments on employment and earnings suggest that at least two of the treatments--JSA only and JSA plus the reemployment bonus--increased claimants' short-run earnings. For these two treatments, earnings impact estimates based on interview data were positive and statistically significant for the first two quarters in the year after the initial UI claim. The earnings impact estimates based on wage records for the JSA plus reemployment bonus treatment were also positive and significant for the first calendar quarter after the initial UI claim (see Table 3). Employment impact estimates (not reported in the table) were also positive and significant for the

same period. The timing of these impacts suggests that they arose primarily because the treatments promoted early reemployment.

We also investigated another short-run employment and earnings issue, the impact of the treatments on the characteristics of the first post-UI job. This is an important issue, because, by promoting rapid reemployment, the treatments might have prompted claimants to accept jobs that were less desirable than those obtained by claimants who were not offered special services. An examination of this issue indicates that the early reemployment promoted by the treatments did not entail any sacrifice in hourly wages or hours worked. In fact, the treatments appear to have led to modest increases in hourly wage rates in post-UI jobs (see Table 3).

We also looked at long-run employment and earnings impacts, because the impacts of training receipt were expected to occur in the longer run. These estimates, based on wage records data, showed no statistically significant treatment impacts over the six-year follow-up period (beyond those observed in the initial quarters following the UI claim). However, variation in claimants' earnings was quite large, so modest longer-run earnings impacts consistent with the UI impact estimates could still have occurred. Similarly, a relatively small number of claimants participated in training, so the impacts of training would need to be quite large to be detected through treatment-control comparisons.

For this reason, we examined the earnings experiences of trainees directly to determine whether the pattern of earnings suggested that training may have had an impact not detected in the treatment-control comparisons. This analysis showed that trainees who participated in classroom-based occupational skills training had relatively low earnings initially, while they participated in training, but that they had relatively higher earnings in later periods (relative to their base period earnings, as compared with similar claimants not offered training). Claimants who participated in on-the-job training had substantially higher earnings throughout the six-year follow-up period. Although these impact estimates could be biased, because the analysis could not completely control for unobserved factors that affect self-selection of training participants, the analysis suggests that both classroom (occupational skills) and on-the-job training did enhance trainees' earnings.

TABLE 3

ESTIMATED TREATMENT IMPACTS ON EARNINGS AND POST-UI WAGES

	JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus	Control Group Mean
Earnings (Dollars)				
Interview Data				
Claim quarter 1	125 **	82	160 **	687
Claim quarter 2	263 **	103	278 ***	1,945
Claim quarter 3	171	83	131	2,701
Claim quarter 4	49	77	22	3,012
Wage Records Data				
Calendar quarter 1	28	58	176 **	1,638
Calendar quarter 2	75	-23	79	2,174
Calendar quarter 3	101	47	46	2,507
Calendar quarter 4	31	28	79	2,517
Post-UI Wages				
Percent Change in Post-UI Relative to Pre-UI Hourly Wage[a]	0.041 **	0.030 **	0.041 **	

NOTE: Quarters for interview data are defined relative to the date of UI claim. That is, quarter 1 is the first three months following the date of claim, quarter 2 is the next three months, and so on. Quarters for the wage records data are calendar quarters beginning with the first full quarter after the date of UI claim.

[a]Data for this variable came from the interview.

* Statistically significant at the 90-percent confidence level for a two-tailed test.
** Statistically significant at the 95-percent confidence level for a two-tailed test.
*** Statistically significant at the 99-percent confidence level for a two-tailed test.

Benefit-Cost Analysis

An important question for any potential program or policy is whether the benefits of offering services exceed their costs. We examined this question for the three treatments tested in the demonstration by looking at benefits and costs from the perspective of claimants, the government, and society as a whole. For example, reductions in UI benefit receipt represent a cost to claimants,

a benefit to the government, and neither a benefit nor a cost to society, since UI payments are transfers from one sector of society to another. The analysis considered *net* benefits (including gains in earnings and taxes paid) and *net* costs, relative to the existing service system.

In terms of costs, the gross costs of providing the three treatments were estimated at $169 per claimant for the JSA-only treatment, $491 per claimant for the JSA plus training or relocation treatment, and $299 per claimant for the JSA plus reemployment bonus treatment. Because some reemployment services are already provided to UI claimants under the existing service system, the net cost of providing these treatments was lower: $155 for the first treatment, $377 for the second, and $276 for the third.

The results of the benefit-cost analysis indicated that each of the treatments offered net benefits to society as a whole and to claimants, when compared with existing services (see Table 4).[6] The JSA-only and JSA plus reemployment bonus treatments also led to net gains for the government sector as a whole and to the Labor Department agencies that actually offer the services--the reductions in UI benefits outweighed the net cost of providing additional services to claimants. Overall, net benefits were similar for these two treatments, and the JSA plus training/relocation treatment was more expensive than the other two from all perspectives.

POLICY ANALYSIS

The demonstration showed that the treatments tested in the demonstration could be implemented successfully. Claimants who are likely to experience long spells of unemployment and reduced post-UI earnings can be identified and provided with services early in their unemployment spell, through the coordinated efforts of the UI, ES, and JTPA systems. Moreover, each treatment

[6]The net benefits to society occur largely because it is assumed that claimants' increased employment and earnings represent a net increase in output; that is, the more rapid reemployment of claimants does not displace the employment of other individuals. This no-displacement assumption seems reasonable given the strength of the New Jersey economy at the time of the study.

TABLE 4

BENEFIT-COST COMPARISON WITH EXISTING SERVICES
(Dollars per Claimant)

Perspective	JSA Only	JSA Plus Training/Relocation	JSA Plus Reemployment Bonus
Society	581	41	565
Claimants	407	200	400
Government	175	-159	165
Labor Department	52	-219	45
Other Government	123	60	120

NOTE: Entries are net benefits (the sum of benefits minus costs) relative to existing services.

led to reductions in the lengths of unemployment spells and to concomitant increases in earnings and reductions in UI benefits received. All three treatments offered net benefits to society as a whole and to claimants, when compared with existing services. The JSA-only and JSA plus reemployment bonus treatments also led to net gains for the government sector as a whole and to the Labor Department agencies that actually offered the services.

Overall, these generally positive findings suggest that the demonstration treatments represent potentially useful reemployment policies that can be directed toward UI claimants. However, before replicating these policies it is important to consider several other evaluation findings. These findings relate to targeting services, applying participation requirements, promoting interagency coordination, and selecting reemployment services.

Targeting Services

Who should receive services is an important question for any reemployment strategy. The eligibility definition used in the demonstration attempted to target services toward displaced workers who would experience reemployment difficulties. In general, this objective was achieved, although some individuals selected for the demonstration presumably did not need services because they were

eventually recalled by their former employers. The remainder covered the spectrum of permanently separated workers, from those who had marketable skills and needed few, if any, services to those who faced major reemployment difficulties.

Analyses of the impacts of the treatments through simulations of worker profiling systems targeted exclusively to claimants with the highest probabilities of UI benefit exhaustion suggest that this approach to targeting reemployment services is useful. Workers with high probabilities of UI benefit exhaustion appear to benefit from the services--that is, UI benefit receipt is reduced for this group and there is also some evidence that the number of weeks worked in the benefit year is increased. While the differences are not statistically significant, the UI impacts also appear larger than the impacts for workers with lower probabilities of benefit exhaustion. This suggests that targeting services on workers with high probabilities of UI benefit exhaustion is a relatively efficient way to provide services. Services are directed to a group of dislocated workers who can benefit more from the services than a random group of dislocated workers, thereby generating relatively large savings in UI receipt for the given level of expenditures on services.

Applying Participation Requirements

The UI system requirement that claimants report for the initial job-search assistance services appears to have been an important element of the treatments. Moreover, evidence from the evaluation suggests that UI and ES staff implemented this requirement successfully. Individuals who did not report and who continued to claim benefits were, in most cases, identified and contacted for followup. Thus, these reporting requirements and the compliance process probably contributed to the increase in service receipt and to the impacts of the treatments on UI receipt and earnings. The legislation authorizing the worker profiling and reemployment services systems currently being implemented mandates such requirements.

Promoting Interagency Coordination

An important element of the New Jersey demonstration was that it relied on the coordinated efforts of the UI, ES, and JTPA systems to identify eligible claimants and to provide them with services. To be successful, this coordination required strengthening linkages among these agencies at both the local service delivery and central office level. These linkages appear to have been strengthened in the New Jersey demonstration both through the development of an automated system linking UI and ES and through the enthusiasm and ability of staff at the local and central levels to work well together. Getting staff to work well together, however, required a high degree of commitment and involvement by top agency officials and key central office staff. Similar efforts are likely to be necessary in any future program, particularly during program implementation.

Selecting Services

The findings summarized earlier indicate that each component of the treatments--job search assistance, training, and the reemployment bonus--contributed to the impacts on UI receipt and earnings. Job-search assistance contributed to the short-run UI and earnings impacts that occurred early in individuals' claim spells, a period in which intensive job-search assistance was provided. Some impacts of this treatment component were also observed in the second year after the initial UI claim. The reemployment bonus, offered in conjunction with job-search assistance, led to larger short-run UI and earnings impacts than were observed for job-search assistance only. Training, both classroom and on-the-job, appeared to enhance the trainees' long-run earnings, although the evidence is weaker than it is for the other treatments.

Each treatment component appeared to contribute to impacts on UI receipt and earnings, but the benefit-cost analysis provides the strongest support for the JSA-only treatment. This treatment offered net benefits to society as a whole and to claimants, when compared with existing services. It also led to net gains to the government sector as a whole and to the Labor Department agencies that offered services. Although the offer of the reemployment bonus generated additional UI savings,

it did not offset the cost of the bonus, nor were the gains in earnings sufficiently larger than those from the JSA-only treatment to make a difference in the benefit-cost comparisons. Overall, the JSA-only and job-search assistance plus reemployment bonus treatments had very similar benefit-cost outcomes from all perspectives. The results from the New Jersey demonstration suggest that a reemployment bonus offer does not appear to improve labor-market outcomes sufficiently to make the *combination* of mandatory job-search assistance plus the bonus offer a more successful treatment than mandatory JSA alone.[7] The benefit-cost findings also indicated that, because the cost of training was high (even though a small percentage of individuals received training), adding the training or relocation assistance offer to the basic JSA services raised costs to the government without generating sufficient UI savings or taxes to offset these costs. However, these findings should not be viewed as indicating that training should not be offered. Training, although expensive to the government, may be the only option to improve the earnings of individuals without marketable skills, for whom the treatments offered in New Jersey had little impact.

[7]Findings from other demonstrations of reemployment bonuses that did not include mandatory job-search assistance suggest that a reemployment bonus can yield net benefits to society, but that, from the standpoint of the UI system, the reductions in UI benefits generated by the bonus offer are largely offset by the cost of the bonus itself (see Corson et al. 1992).

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